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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Polit.* lib. i. cap. 1. Not.

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PLATES.

Errata.

P. 172 line 12, and p. 179, line 6, *for* [Plate II.] *read* [Plate I.]

P. 231, line 18, *for* M. Ertel, *read* M. Reichenbach.

P. 497, 498, throughout the notice of M. Frémy's researches on the oxides of lead, *for* plombic acid, plombates, and plombites, *read* plumbic acid, plumbates, and plumbites, respectively.

THE

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[THIRD SERIES.]

JANUARY 1843.

I. *On the Efficacy of Steam as a means of producing Electricity, and on a curious Action of a Jet of Steam upon a Ball.* By W. G. Armstrong, *Esq.**

THE experiments which I have made on the electricity of steam, since the date of my last communication to the Philosophical Magazine †, have completely confirmed the conclusion which I had then arrived at, that the excitation of electricity takes place at the point where the steam is subjected to friction; and by the improvements I have effected in the mode of discharging the steam, I have so amazingly increased the energy of the effects, that I question whether any electrical machine has yet been constructed capable of producing as much electricity as my electrical boiler. At all events the boiler has been proved to possess upwards of *seven* times the efficiency of an excellent machine which has a plate of three feet in diameter and which was worked at the rate of seventy revolutions in a minute when its power was tried. The comparison was made by means of a discharging electrometer, and the following tabular statement will convey some idea of the quantity of electricity produced in each case.

Capacity of the jar of the electrometer	$\frac{1}{2}$ a gallon nearly.
Extent of coated surface on the two sides taken together	198 square inches.
Distance of the balls of the electrometer from each other	$\frac{1}{3}$rd of an inch.
Number of discharges obtained per minute when the instrument was connected with the prime conductor of the machine	29

* Communicated by the Author.   † Phil. Mag. S. 3, vol. xx. p. 5.

Number of discharges obtained per minute when it was applied to the insulated boiler	220

The discharges were so exceedingly rapid when the electrometer was connected with the boiler, that it was difficult to count them with accuracy, but the number I have inserted is assuredly not overstated.

The boiler is a wrought-iron cylinder, with rounded ends, and measures three feet six inches in length, and one foot six inches in diameter. It rests upon an iron frame, containing the fire, and the whole apparatus is supported upon glass legs to insulate it. The application of the fire is unfortunately very imperfect, in consequence of which the boiler will not maintain, for any considerable time, the discharge of steam which is requisite to produce the effects I have mentioned, but a short interval of quiescence suffices to restore the pressure, and to render the boiler again ready for action.

It is much more convenient and effectual to collect electricity from the boiler than from the steam-cloud, but in order to obtain the highest effect from the boiler the electricity of the steam must be carried to the earth by means of proper conductors.

Notwithstanding the enormous dissipation of electricity which is occasioned, when the tension is great, by the dust and effluvia of the fire, and by the angular parts of the apparatus, I can draw sparks, twelve inches long, with great rapidity from the rounded ends of the boiler; and if a projecting ball of proper dimensions were attached to the apparatus, much longer sparks would probably be obtained.

I find it essential to a high development of electricity, that the steam should be discharged with a slight intermixture of water, although, from a cause which it is needless to explain, this did not appear to be the case in the experiments which I formerly made with a gun-metal generator.

A piece of hard wood, such as ebony or partridge wood, is the best material I have yet tried in which to make the discharging passage, but it is chiefly by prefixing to the wooden channel, a brass cap, of very peculiar construction, that I have been enabled to obtain the present powerful effects. The piece of wood containing the discharging passage, is, for the convenience of fixing, formed into a plug such as is represented in fig. 1. The brass cap, to which I have just alluded, is affixed to the smaller extremity of the plug, and is shown in that position in the drawing. Fig. 2 is a longitudinal section of the same, drawn to the full size, and exhibits the hole through the wood, and the internal structure of the cap. The

arrows indicate the course of the steam which passes, in the first instance, through a lateral slit or saw-cut in the brass,

Fig. 1.

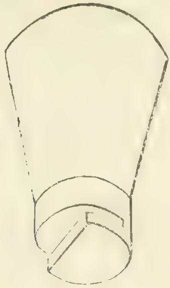


Fig. 3.

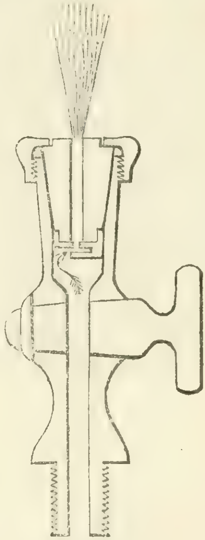
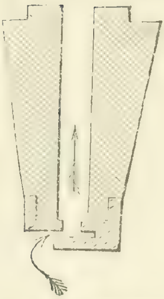


Fig. 2.



about $\frac{1}{50}$th of an inch wide, then through a circular hole in the centre of the cap about $\frac{1}{10}$th of an inch in diameter, and finally through the wooden channel from which it is ejected into the air. The passage through the wood is of cylindrical form, and of somewhat larger diameter than the circular hole in the centre of the brass. Fig. 3 is a stop-cock, with a socket to receive the plug, which is kept firmly down by a screw-nut at the top.

Several cocks of this description, each fitted with a wooden plug, such as I have described, are screwed into an iron vessel communicating with the boiler, and in which the proper quantity of moisture, to be carried out with the steam, is deposited by condensation. The steam is used at a pressure of about 70 pounds on the square inch, and is discharged horizontally in diverging jets. Each jet affords quite as much electricity as a good electrical machine of ordinary dimensions; and when it is considered that a boiler of evaporating power equal to that of a locomotive engine would be adequate to sustain hundreds of such jets, an idea may be formed of the

prodigious evolution of electricity which it is practicable to obtain by the agency of steam.

Although it is perfectly clear that the electricity is excited in the discharging passage where the steam is exposed to violent friction, yet as the mode of ejection which I have described is neither characterized by peculiar violence of friction, nor by great extent of rubbing surface acted upon by the steam, I feel great difficulty in accounting for its extraordinary efficacy, upon the supposition that *friction* is the exclusive cause of the excitation.

In the course of my experiments I have observed a very singular effect of a jet of steam, which, as far as I am aware, has never been noticed in any publication, and which I therefore take this opportunity of mentioning, although it is quite unconnected with electricity.

If a ball A, fig. 4, be immersed in a vertical jet of high-

Fig. 4.

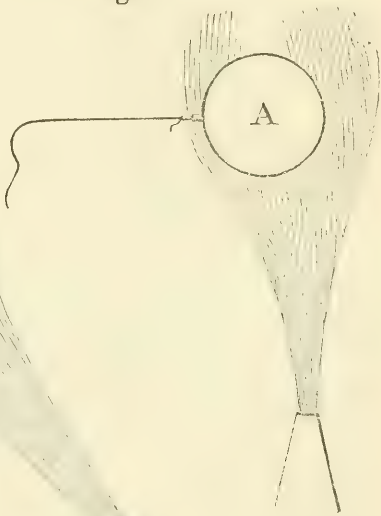
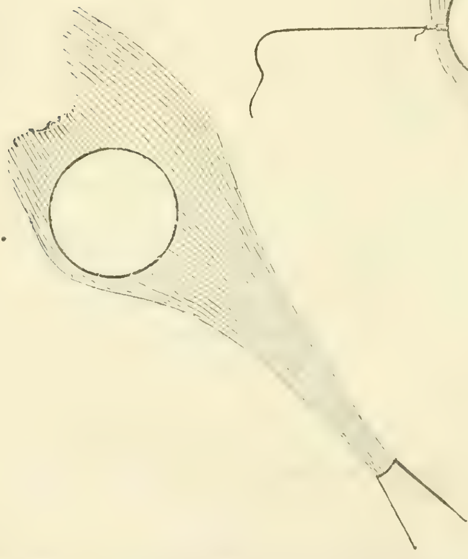


Fig. 5.



pressure steam, the ball will remain suspended in the jet without any other support than it derives from the steam, and if it be pulled to one side by means of a string as shown in the figure, a very palpable force will be found requisite to draw it out of the jet. The experiment may be varied, and rendered ex-

ceedingly striking, by discharging the steam obliquely as in fig. 5, in which case the ball will take up its position at a greater distance from the orifice, but will still be sustained in the current, notwithstanding that gravity in this instance acts *at an angle to the jet.* A hollow globe made of thin brass or copper and from two to three inches in diameter, answers very well for the purpose, where the steam is discharged from an aperture not less than $\frac{1}{20}$th of a square inch in area.

In the well-known experiment of supporting a ball upon the summit of a jet of water, the ball merely reposes in the hollow formed by the liquid in the act of turning over to fall to the ground, which is very different from being sustained in the current as it is in the case of the steam.

Jesmond Dene, Newcastle-upon-Tyne, Nov. 15, 1842. WM. GEO. ARMSTRONG.

II. *On the Action of the Rays of the Solar Spectrum on Vegetable Colours, and on some new Photographic Processes. By* Sir JOHN F. W. HERSCHEL, *Bart., K.H., F.R.S.**

[With a Plate.]

149†. IN my paper on the "Chemical Action of the Solar Spectrum on preparations of Silver and other substances," read to the Royal Society in February 1840, and of which the present communication is intended as a continuation or supplement, some experiments on the effect of the spectrum on the colouring matter of the *Viola tricolor*, and on the resin of guaiacum are described, which the extreme deficiency of sunshine during the summer and autumn of the year 1839 prevented me from prosecuting efficiently up to the date of that communication. The ensuing year 1840 was quite as remarkable for an excess of sunshine as its predecessor for the reverse. Unfortunately the derangements consequent on a change of residence prevented my availing myself of that most favourable conjuncture, and it was not till the autumn of that year that the inquiry could be resumed. From that time to the present date it has been prosecuted at intervals as the weather would allow, though owing to the almost unprecedented continuance of bad weather during the whole of the past summer and autumn (1841), it has of late been almost wholly suspended ‡. In photographic processes, where silver and other metals are used, the effect of

* From the Philosophical Transactions, 1842, p. 181; having been received by the Royal Society June 15, and read June 16, 1842.

† The paragraphs, for convenience of reference, are numbered in continuation of those of the previous paper referred to in the text [an abstract of which will be found in Phil. Mag. S. 3, vol. xvi. p. 331.—EDIT.].

‡ This was written in April 1842, since which a repetition of the season of 1840 seems to have commenced.

light is so rapid that the state of the weather, as to gloom or sunshine, is of little moment. It is otherwise in the class of photographic actions now to be considered, in which exposure to the concentrated spectrum for many hours, to clear sunshine for several days, or to dispersed light for whole months, is requisite to bring on many of the effects described, and those some of the most curious. Moreover, in such experiments, when unduly prolonged by bad weather, the effects due to the action of light become mixed and confounded with those of spontaneous changes in the organic substances employed, arising from the influence of air, and especially of moisture, &c., and so give rise to contradictory conclusions, or at all events preclude definite results, and obscure the perception of characters which might serve as guides in an intricate inquiry, and afford hints for the conduct of future experiment. It is owing to these causes that I am unable to present the results at which I have arrived, in any sort of regular or systematic connexion; nor should I have ventured to present them at all to the Royal Society, but in the hope that, desultory as they are, there may yet be found in them matter of sufficient interest to render their longer suppression unadvisable, and to induce others more favourably situated as to climate, to prosecute the subject.

150. The materials operated on in these experiments have been for the most part the juices of the flowers or leaves of plants, expressed, either simply, or with addition of alcohol, or under the influence of other chemical reagents. Some few resinous and dyeing substances have also been subjected to experiment, but with less perseverance than the obvious practical importance of this branch of the subject might demand, except in the case of guaiacum, whose relations to light, heat, and chemical agents are exceedingly remarkable and instructive, for which reason, as well as because some of these relations have been treated of in my former paper, I shall commence the account of my later experiments with those made on this substance. But in the first place it is necessary to state that the apparatus used for forming, concentrating, and fixing the spectrum, was the same with that described in Art. 67. of that paper; the prism being that of flint-glass by Fraunhofer, there mentioned; the area of the section of the incident sunbeam = 1·54 square inch, and the dimensions of the principal elements of the luminous spectrum, identical with those recorded in §. 70, so that the following results, when numerically stated (in measures of which the unit is one-thirtieth of an inch), will be comparable with those previously described. To spare reference, however, it may be here mentioned that the diameter of the sun's image in the focus of the achro-

matic lens used is 7·20 of such thirtieths; and that the extent of the visible spectrum corrected for the sun's semidiameter at either end, equals 53·92 thirtieths, of which* 13·30 are considered as reckoned negatively to the extreme visible red from a fiducial point or centre corresponding to the mean yellow ray; and 40·62 positively, from the same centre to the terminal violet, both as seen through a certain standard blue glass, which lets both extremes pass freely and insulates the mean yellow with considerable precision. The correction for the sun's semidiameter has been applied in what follows to all measures up to *terminations of spectra*, unless where the contrary is expressed. Maxima and minima of action, and neutral points neither require nor admit this correction.

Guaiacum.

151. A solution of this resin in alcohol, spread evenly on paper, gives a nearly colourless ground. A slip of this paper exposed to the spectrum is speedily impressed with a fine blue streak over the region of the violet rays, and far beyond, as described in Art. 92. If the paper during this action be carefully defended from extraneous light, this is the only perceptible effect; but if dispersed light be admitted, the general ground of the paper is turned to a pale brownish green, with exception of that portion on which the less refrangible rays fall, which, by their agency, is defended from the action of the dispersed light and preserves its whiteness, as in the case of the argentine paper described in Art. 60. The spectrum, therefore, ultimately impressed, consists of two portions similar to those described in Art. 93, and of nearly the same extent, that is to say, a white or pale yellowish portion having its maximum of intensity at 0·0, and extending from − 11·9 (corrected for the sun's semidiameter) to + 12·0, or thereabouts, at which point the character of the action changes, and a blue, of a somewhat smoky gray cast, commences, which attains a maximum at + 40·0, thence degrades to an intermediate minimum at + 47·0, attains a second and much stronger maximum at + 61·0, and ceases at 72·4. The precise numbers vary materially in different specimens and with the length of exposure. The type of this spectrum, of its natural length, is represented in Plate I. fig. 1, in which the abscissæ being measured along the length of the spectrum, from the fiducial centre Y both ways, the ordinates express the intensities of photographic action at each corresponding point, as estimated from the amount of colour induced or prevented. In this type the portion corresponding to the less refrangible rays is represented by negative values of the or-

dinate agreeably to Art. 93, where it is shown that these rays not only prevent the blue colour from being produced by the more refrangible ones, but destroy it when so produced. Another specimen gave the following dimensions: $\mathrm{Y}a = -11{\cdot}4$ $\mathrm{Y}b = -9{\cdot}5$, $\mathrm{Y}c = +30{\cdot}0$, $\mathrm{Y}d = +61{\cdot}0$, $\mathrm{Y}e = +80{\cdot}4$, and this is the greatest extent of action I have hitherto observed.

152. A portion of the same paper was exposed, dry, to an atmosphere of chlorine considerably diluted with common air, which imparted to it a pale, dirty, greenish yellow hue. Being thence transferred immediately to the spectrum, the result was not a little remarkable. The whole spectrum, the green excepted, was impressed in faint tints nearly corresponding to the natural ones. The red was evident—the yellow dilute and nearly white—the blue a fine sky-blue, while beyond the violet succeeded a train of somewhat greenish darkness. These tints proved fugitive, and in twenty-four hours were nearly obliterated.

153. When paper fresh washed with tincture of guaiacum and still wet is exposed to chlorine, it instantly acquires a fine and full Prussian blue colour, which however passes speedily to brown if the action be prolonged. The colour is difficult to preserve in its full intensity, and fades considerably in drying, becoming at the same time somewhat greenish. Exposed wet to the spectrum, it is found to have become much more sensitive, and is immediately attacked with great energy by the red rays, which destroy the blue colour, converting it to a brownish or reddish yellow. The action extends rapidly up the spectrum as far as the extreme violet, in which ray, however, the tint impressed or left undestroyed passes to a hue partaking of violet, and indicating by the change what ought probably to be regarded as a neutral point at $+12{\cdot}0$. The impressed spectrum (corrected for semidiameter) commences at a, fig. 2, at $-13{\cdot}4$; the maximum b of the positive action occurs at $-9{\cdot}0$, the neutral point c at $+12{\cdot}0$, the maximum d of negative action at $+33{\cdot}0$, and the sensible termination e of the impression at $+60{\cdot}0$.

154. The action of gaseous chlorine is too energetic to be easily arrested at the proper point, besides which this gas also acts powerfully on the alcohol employed. To obviate these inconveniences, paper thoroughly impregnated with guaiacum by washing with the tincture, and drying in a gentle heat, was steeped in weak aqueous solution of chlorine, by which process it slowly acquired a beautiful and pure celestial blue colour. It is very sensitive, and may be conveniently used for copying engravings, &c., which it does with this singularity,

that the picture penetrates the paper and appears on the back of very nearly the same intensity as on the face *. Indeed, if the picture be over-sunned the back will exhibit a perfect impression, while the face is spoiled, which produces a very strange effect: exposed to the spectrum, the blue colour is converted to a pale reddish yellow in the region of the less refrangible rays, and simply whitened in the more refrangible region. The action, when prolonged till the light seems to have no further influence, extends from −12·4, corrected for semidiameter, to +40, or thereabouts, where it dies away insensibly. The maximum of photographic action occurs at −8·7, and some trace of a minimum is perceptible at +11·5. Photographs taken on this paper, or spectra impressed on it, are fugitive—lose much of their force and beauty in a few days, and at length vanish altogether.

155. When paper is washed with a solution of guaiacum in soda it acquires a green colour, though the solution itself is brown. By inclining the paper and carrying the wash always from below upwards, a very even tint may be obtained. The excess of liquid being blotted off, aqueous solution of chlorine was poured over it (on a slope) till all the alkali was saturated, and the liquid ran off smelling strongly of chlorine. Thus was produced a paper (No. 1168.) very evenly tinted, and varying in colour from a deep, somewhat greenish, to a fine celestial blue, according to the strength of the solutions employed. It is very sensitive, and is attacked with especial energy by rays in the spectrum, ranging from −11·4 to +11·4 with a maximum at −9·0, the type being as in fig. 3.

156. When paper so prepared is exposed, wet, to a temperature of 212° Fahr., it is immediately discoloured, the green changing to a sere or brownish yellow. The same change is produced after some little time at a temperature of 190°, and still more slowly, though yet completely, at 180°. At 175° the discoloration is incomplete and very slow; and below that temperature the colour is not affected. If the paper be perfectly dried in a temperature gradually raised to 212°, the discoloration requires a considerably higher temperature, ranging from 220° to 275°, according to the time of exposure, being very slow at the former limit and almost immediate at the latter. These changes are independent of the action of light, being produced under mercury.

157. The destruction by heat of the green or blue colour superinduced on guaiacum by the more refrangible rays of light, was noticed by Wollaston, and it would seem, on a

* For another remarkable case of this kind see the Postscript to this paper.

consideration of his experiments and of those described in the last article, that nothing further is requisite for operating the change from the green or blue to the yellow state, than the assumption of a certain temperature dependent on its state of dryness, and varying according to that state between the limits of 180° and 280°. Nevertheless, if we consider that the same change is produced by rays of the spectrum which are very far from being the *hottest*, while yet the extra-spectral thermic rays, under precisely the same circumstances of exposure, produce no such effect, though far surpassing in mere calorific power those which do, we shall see reason to doubt the sufficiency of this view of the matter. The following experiments were therefore instituted with a view to its further elucidation.

158. A slip of the paper No. 1168 was moistened and subjected in clear sunshine to the action of the spectrum. The colour was discharged from the region occupied by the less refrangible luminous rays, as described in Art. 155. At the same time, the more distant thermic rays beyond the spectrum produced their proper effect, in evaporating the moisture from those portions on which they fell; so that in due time the *heat-spots* δ and γ became apparent (see Art. 136.), the former very distinctly, the latter perceptibly. The spot β (which is remarkable) was scarcely if at all formed. So long then as the paper continued moist and remained under the influence of the thermic rays, the appearances were those of *a diminution of colour* (Art. 131.), operated by the thermic rays δ and γ. But the discoloration in these points was only apparent, for as the paper dried these *heat-spots* disappeared, leaving its colour quite unchanged at those points; while the photographic impression really produced within the visible spectrum, remained and went on increasing in intensity. The non-luminous thermic rays, therefore, though clearly shown to have been active *as heat*, were yet incapable of effecting that peculiar chemical change which other rays much less copiously endowed with heating power, were all the while producing.

159. It may be objected to this, that no proof is afforded in the above-related experiment, that any part of the paper actually attained a temperature of 180° or more; that in consequence no discoloration due to the action of heat (*quoad heat*) was produced; and that the discoloration which did take place was *sui generis*, and originated with the *light and not the heat* of that part of the spectrum to which it corresponded. A slip of the same paper (1168.) was therefore exposed dry to the spectrum in such a way as to leave its back accessible; and an iron heated below redness was then ap-

proached to it so as *just not* to discolour the paper. Under such circumstances it might be expected that the additional heat thrown on the paper in the region of the thermic rays would turn the scale in their favour at their points of greatest intensity, and give ocular proof of their action by a decided discharge of colour at those points. But no such result was obtained, nor could I succeed in rendering visible any of the heat-spots α, β, γ, δ, even when the heated iron was brought so near as to produce a commencement of discoloration over the whole of that region of the paper where they ought to have shown themselves.

160. On the other hand, a remarkable, but by no means an unexpected influence, was exercised by the heat so thrown on that part of the paper where the less refrangible rays fell, and where the discoloration was in progress under their agency. For it was observed that, under these circumstances, the discoloration in question went on with much greater rapidity, so much so indeed, that the same amount of it, which without extraneous heat would have required twenty minutes or half an hour's exposure to the spectrum to produce, was now produced in two or three minutes. Obscure terrestrial heat, therefore, is shown to be capable of *assisting* and *being assisted* in operating this peculiar change, by those rays of the spectrum, whether luminous or thermic, which occupy its red, yellow, and green regions; while on the other hand it receives no such assistance from the purely thermic rays beyond the spectrum, acting under precisely similar circumstances, and in an equal state of condensation.

161. When heat was similarly applied by radiation from behind, and from a non-luminous source, over the *more* refrangible region of a spectrum thrown on paper simply washed with tincture of guaiacum and not previously blued either by chlorine or by light, the blue colour induced in the more refrangible rays was still produced, and of the same tint in the same points as if no heat had acted. This effect, the contrary to what the previous experiment would have led to expect, shows how little any reasonings on these points enable us at present to anticipate experience.

162. The discharge of colour from blued guaiacum by mere heat, has been shown above (Art. 156.) to take place at a much lower temperature in the presence of moisture than when dry; and a similar destruction of colour, under similar circumstances, takes place with many other vegetable preparations. Paper, for instance, coloured with the juice of the *Viola tricolor* (Art. 90.), is speedily whitened in the dark, while wet, by the heat of boiling water, though dry heat does

not affect it. And under the action of the spectrum it is discoloured (though much more slowly) by the same, or nearly the same rays which are effective in the case of guaiacum. The colour of paper tinged with the juice of the common red stock is not affected when dry by any heat short of what suffices to scorch the paper, but when wet (as when exposed to steam) it is speedily discharged. There are few, if any vegetable colours indeed which long resist the combined effects of heat and moisture, even when light is excluded, still less when admitted*.

Of the Colours of Flowers in general under the action of the Spectrum.

163. In operating on the colours of flowers I have usually proceeded as follows:—the petals of the fresh flowers, or rather such parts of them as possessed a uniform tint, were crushed to a pulp in a marble mortar, either alone, or with addition of alcohol, and the juice expressed by squeezing the pulp in a clean linen or cotton cloth. It was then spread on paper with a flat brush, and dried in the air without artificial heat, or at most with the gentle warmth which rises in the ascending current of air from an Arnott stove. If alcohol be not added, the application on paper must be performed immediately, since exposure to the air of the juices of most flowers (in some cases even for but a few minutes) irrecoverably changes or destroys their colour. If alcohol be present this change does not usually take place, or is much retarded; for which reason, as well as on account of certain facilities afforded by its admixture in procuring an even tint (to be presently stated), this addition was commonly, but not always made.

164. Most flowers give out their colouring matter readily enough, either to alcohol or water. Some, however, as the Eschnolzias and Calceolarias, refuse to do so, and require the addition of alkalies, others of acids, &c. When alcohol is added, it should, however, be observed that the tint is often, apparently, much enfeebled, or even discharged altogether, and that the tincture, when spread on paper, does not reappear of its due intensity till after complete drying. The temporary destruction of the colour of the blue heartsease by alcohol has been noticed in my former paper (Art. 90.), nor is

* On the effects of light, air, and moisture at common temperatures, as discolouring agents on several dyeing materials, I may refer to M. Chevreul's elaborate memoir (*Acad. R. des Sciences*, tom. xvi.). M. Chevreul's experiments, however, relate to the action of light simply as it comes from the sun without prismatic separation, and have therefore little or nothing in common with the objects of this paper.

that by any means a singular instance. In some, but in very few cases, it is destroyed, so as neither to reappear on drying, nor to be capable of revival by any means tried. And in all cases long keeping deteriorates the colours and alters the qualities of the alcoholic tinctures themselves, so that they should always be used as fresh as possible.

165. If papers tinged with vegetable colours are intended to be preserved, they must be kept perfectly dry and in darkness. A close tin vessel, the air of which is dried by quicklime (carefully enclosed in double paper bags, well pasted at the edges to prevent the dust escaping), is useful for this purpose. Moisture (as already mentioned, especially assisted by heat) destroys them for the most part rapidly, though some (as the colour of the *Senecio splendens*) resist obstinately. Their destructibility by this agency, however, seems to bear no distinct relation to their photographic properties.

166. This is also the place to observe that the colour of a flower is by no means always, or usually, that which its expressed juice imparts to white paper. In many cases the tints so imparted have no resemblance to the original hue. Thus, to give only a few instances, the red damask rose of that intense variety of colour, commonly called by florists the Black Rose, gives a dark slate blue, as do also the clove carnation and the black holyoak; a fine dark brown variety of Sparaxis gave a dull olive green; and a beautiful rose-coloured tulip, a dirty bluish green; but perhaps the most striking case of this kind is that of a common sort of red poppy (*Papaver Rheum*?), whose expressed juice imparts to paper a rich and most beautiful blue colour, whose elegant properties as a photographic material will be further alluded to hereafter*.

167. This change of colour is probably owing to different causes in different flowers. In some it undoubtedly arises from the escape of carbonic acid, but this as a general cause for the change from red to blue, has, I am aware, been controverted†. In some (as is the case with the yellow Ranunculi) it seems to arise from a chemical alteration depending on absorption of oxygen; and in others, especially where the expressed juice coagulates on standing, to a loss of vitality or disorganization of the molecules. The fresh petal of a single flower, merely crushed by rubbing on dry paper, and instantly dried, leaves a stain much more nearly approximating to the original hue. This, for example, is the only way in

* A semi-cultivated variety was used, having dark purple spots at the bases of the petals. The common red poppy of the chalk (*Papaver hybridum*) gives a purple colour much less sensitive and beautiful.

† Nicholson's Journal.

which the fine blue colour of the common field Veronica can be imparted to paper. Its expressed juice, however, quickly prepared, when laid on with a brush, affords only a dirty neutral gray, and so of many others. But in this way no even tint can be had, which is a first requisite to the experiments now in question, as well as to their application to photography.

168. To secure this desirable evenness of tint, the following manipulation will generally be found successful. The paper should be moistened at the back by sponging and blotting off. It should then be pinned on a board, the moist side downwards, so that two of its edges (suppose the right-hand and lower ones) shall project a little beyond those of the board. The board being then inclined twenty or thirty degrees to the horizon, the alcoholic tincture (mixed with a very little water, if the petals themselves be not very juicy) is to be applied with a brush in strokes from left to right, taking care *not* to go over the edges which rest on the board, but *to* pass clearly over those which project, and observing also to carry the tint from below upwards by quick sweeping strokes, leaving no dry spaces between them, but keeping up a continuity of wet surface. When all is wet, cross them by another set of strokes from above downwards, so managing the brush as to leave no floating liquid on the paper. It must then be dried as quickly as possible over a stove, or in a current of warm air, avoiding, however, such heat as may injure the tint. The presence of alcohol prevents the solution of the gummy principle, which, when present, gives a smeary surface; but the evenness of tint given by this process results chiefly from that singular intestine movement which always takes place when alcohol is in the act of separation from water by evaporation—a movement which disperses knots and blots in the film of liquid with great energy, and spreads them over the surrounding surface.

169. The action of the spectrum, or of white light, on the colours of flowers and leaves, is extremely various, both as regards its total intensity and the distribution of the active rays over the spectrum. But certain peculiarities in this species of action obtain almost universally.

1st. The action is *positive*, that is to say, light destroys colour; either totally, or leaving a residual tint, on which it has no further, or a very much slower action. And thus is effected a sort of chromatic analysis, in which two distinct elements of colour are separated, by destroying the one and leaving the other outstanding. The older the paper, or the tincture with which it is stained, the greater is the amount of this residual tint.

2nd. The action of the spectrum is confined, or nearly so, to the region of it occupied by the luminous rays, as contra-distinguished both from the so-called chemical rays, beyond the violet, which act with the chief energy on argentine compounds, but are here for the most part ineffective, on the one hand, and on the other, from the thermic rays beyond the red, which appear to be totally so. Indeed, I have hitherto observed no instance of the extension of this description of photographic action on vegetable colours beyond, or even *quite* up to, the extreme red.

170. Besides these, it may also be observed that the rays effective in destroying a given tint, are, in a great many cases, those whose union produces a colour complementary to the tint destroyed, or at least one belonging to that class of colours to which such complementary tint may be referred. For example, yellows tending towards orange are destroyed with more energy by the blue rays; blues by the red, orange, and yellow rays; purples and pinks by yellow and green rays.

171. These are certainly remarkable and characteristic peculiarities, and must indeed be regarded as separating the luminous rays by a pretty broad line of chemical distinction from the non-luminous; though whether they act *as such*, or in virtue of some peculiar chemical quality of the heat which accompanies them *as heat*, is a point which the experiments on guaiacum, above described, seem to leave rather equivocal. In the latter alternative, chemists must henceforward recognize differences not simply of intensity, but of quality in heat from different sources; of quality, that is to say, not merely as regards degree of refrangibility or transcalescence, but as regards the strictly chemical changes it is capable of effecting in ingredients subjected to its influence.

172. As above stated, these peculiarities, at least the first two, obtain almost universally. Exceptions, however, though very rare, do occur, as will be more particularly mentioned hereafter. The third rule is much less general, and is to be interpreted with considerable latitude; but among its exceptions I have been unable to detect any common principle capable of being distinctly enunciated.

173. Lastly, it requires to be expressly mentioned, that the habitudes of the colours, both of the flowers and leaves of plants, with relation either to white light or to the prismatic rays, vary materially with the advance of the season, and perhaps also with the hour of the day at which they are gathered. Generally speaking, so far as I have been able to observe, the earlier flowers of any given species reared in the open air (provided they are well ripened, i. e. the colour fully deve-

loped) are more sensitive than those produced even from the same plant, at a late period in its flowering, and have their colours more completely discharged by light. As the end of the flowering period comes on, not only the destruction of the colour by light is slower, but residual tints are left which resist obstinately. A very remarkable case of this kind was noticed in *Chryseïs californica*, the earliest flowers of which exhibited in the photograph of their spectrum a well-insulated round spot, eaten away by red rays almost at its extremity, which spot I never was able to reproduce with later flowers from the same root. Those gathered at the end of its flowering also left a residual yellow of extreme obstinacy*, which was by no means the case with the earlier flowers.

174. It would be waste of time to enumerate all the vegetable tints which I have subjected to experiment, comprising most of the ordinary hardy garden and wild flowers of the country. To the rarer and more splendid species which adorn the stoves and greenhouses of florists, I have had little access, a circumstance I much regret, and which leads me to take this opportunity of mentioning, that specimens of paper stained with the juices of highly-coloured, or otherwise remarkable flowers or leaves, either by alcoholic extraction, or by simple expression (if accompanied with the botanical name of the plant used), will be highly acceptable, from whatever quarter received. I shall here set down only those which afforded some ground for special remark, so far as I have yet pushed the inquiry.

Colours of particular Flowers.

175. *Corchorus Japonica.*—The flowers of this common and hardy but highly ornamental plant, are of a fine yellow, somewhat inclining to orange, and this is also the colour the expressed juice imparts to paper. As the flower begins to fade *the petals whiten*, an indication of their photographic sensibility, which is amply verified on exposure of the stained paper to sunshine. I have hitherto met with no vegetable colour so sensitive. If the flowers be gathered in the height of their season, paper so coloured (which is of a very even and beautiful yellow) begins to discolour in ten or twelve minutes in clear sunshine, and in half an hour is completely whitened. The colour seems to resist the first impression of the light, as if by some remains of vitality, which being overcome, the tint gives way at once, and the discoloration when commenced

* Probably, therefore, useful in dyeing. The species is that most commonly cultivated in gardens, with bright yellow petals having orange-coloured bases.

goes on rapidly. *It does not even cease in the dark when once begun.* Hence it happens that photographic impressions taken on such paper, which when fresh are very sharp and beautiful, fade by keeping, visibly from day to day, however carefully preserved from light. Specimens of such photographs (copies of engravings) are submitted with this paper for inspection. They require from half an hour to an hour to complete, according to the sunshine. Hydriodate of potash cautiously applied, retards considerably, but does not ultimately prevent, this spontaneous discharge.

176. Exposed to the spectrum, in about fifteen or twenty minutes the colour is totally destroyed and the paper whitened in the whole region of the green, blue and violet rays, to which therefore the most energetic action is confined, agreeably to the law of complementary tints (Art. 170.). If the action of the spectrum be prolonged, a much feebler whitening becomes sensible in the red, and a trace of it also beyond the violet into the "lavender" rays. In this state the type of the impressed spectrum (in an experiment made on the 7th of April in the present year) was as in fig. 4, indicating three obsolete maxima c, d, e, and a very sudden diminution of the action at b, f, the dimensions being as follows: $\mathrm{Y}a = -9{\cdot}4$, $\mathrm{Y}b = +7{\cdot}1$, $\mathrm{Y}c = +12{\cdot}5$, $\mathrm{Y}d = +23{\cdot}5$, $\mathrm{Y}e = +34{\cdot}0$, $\mathrm{Y}f = +41{\cdot}4$, $\mathrm{Y}g = +59{\cdot}7$. The paper thus impressed was again re-examined on the 2nd of May, or after twenty-five days, during which interval it had been exposed to free air, but only to feeble and dispersed occasional lights. It was found to have undergone a remarkable change, two distinct white spots having become insulated, or nearly so, at the very extremities of the impressed spectrum, the three maxima above indicated having also become much more distinct, and two new, subordinate ones, having begun to show themselves in the faint traces connecting the spots above mentioned with the main impression. The type of the spectrum in this state was as represented in fig. 5, and the places of the several maxima being as follows:—1st, $-10{\cdot}0$; 2nd, $-0{\cdot}5$; 3rd, $+12{\cdot}0$; 4th, $+29{\cdot}0$; 5th, $+40{\cdot}0$; 6th, $+50$::; 7th, $+61{\cdot}0$. The terminal spot at the red extremity was nearly equal in diameter to the sun's image; that at the least refracted end, corresponding in place to rays much beyond the last violet, was smaller, but perfectly distinct, and as it constitutes the only instance I have yet encountered of a *definite* ray in this region of the spectrum*, I have been thus particular in describing the phænomenon.

* Since this was written, other cases, extremely remarkable, among the argentine preparations, have presented themselves. See Art. 214.

177. *Common ten-weeks Stock, Mathiola annua.*—The colour imparted by the petals of the *double variety* of this flower* to alcohol (at least when spread on paper, for it is in great measure dormant in the liquid tincture) is a rich and florid rose-red, varying, however, from a fiery tint almost amounting to scarlet, on the one hand, to a somewhat crimson or slightly purplish red on the other, according to the accidents of its preparation, or the paper used. When fresh prepared it is considerably sensitive, an hour or two of exposure to sunshine being sufficient to produce a sensible discoloration, and two or three days entirely to whiten it. This quality is greatly deteriorated by keeping, but papers prepared with it even after eight or ten months, still with patience yield extremely beautiful photographs, several specimens of which in various states of the tincture are submitted for inspection to the meeting. Exposed to the spectrum, the rays chiefly active in operating the discoloration are found to be those extending from the yellow to the less refrangible red, beyond which rays the action terminates abruptly. Above the yellow it degrades rapidly to a minimum in the blue, beyond which it recovers somewhat, and attains a second but much feebler maximum in the violet rays.

178. Paper stained with the tincture of this flower is changed to a vivid scarlet by acids, and to green by alkalies; if ammonia be used the red colour is restored as the ammonia evaporates, proving the absence of any acid quality in the colouring matter sufficiently energetic to coerce the elastic force of the alkaline gas. Sulphurous acid whitens it, as do the alkaline sulphites; but this effect is transient, and the red colour is slowly restored by free exposure to air, especially with the aid of light, whose influence in this case is the more remarkable, being exactly the reverse of its ordinary action on this colouring principle, which it destroys irrecoverably, as above stated. The following experiments were made to trace and illustrate this curious change.

179. Two photographic copies of engravings taken on paper tinted with this colour were placed in a jar of sulphurous acid gas, by which they were completely whitened, and all traces of the pictures obliterated. They were then exposed to free air, the one in the dark, the other in sunshine. Both recovered, but the former much more slowly than the latter. The restoration of the picture exposed to the sun was completed in

* That imparted by the single flowers is very much less sensitive, as is also that of the dull red or purplish variety, whether double or single. The most florid red double flowers, in the height of their flowering, yield the best colour.

twenty-four hours, that in the dark not till after a lapse of two or three days.

180. A slip of the stained paper was wetted with liquid sulphurous acid and laid on blotting-paper similarly wetted. Being then crossed with a strip of black paper, it was laid between glass plates and (evaporation of the acid being thus prevented) was exposed to full sunshine. After some time the red colour (in spite of the presence of the acid) was considerably restored in the portion exposed, while the whole of the portion covered by the black paper remained (of course) perfectly white.

181. Slips of paper, stained as above, were placed under a receiver, beside a small capsule of liquid sulphurous acid. When completely discoloured they were subjected (on various occasions, and after various lengths of exposure to the acid fumes from half an hour to many days) to the action of the spectrum; and it was found, as indeed I had expected, that *the restoration of colour was operated by rays complementary to those which destroy it in the natural state of the paper*, the violet rays being chiefly active, the blue almost equally so, the green little, and the yellow, orange, and most refrangible red not at all. In one experiment a pretty well-defined red solar image was developed by the *least* refrangible red rays also, being precisely those for which in the unprepared paper the discolouring action is abruptly cut off. But this spot I never succeeded in reproducing; and it ought also to be mentioned, that, according to differences in the preparation not obvious, the degree of sensibility, generally, of the bleached paper to the restorative action of light differed greatly; in some cases a perceptible reddening being produced in ten seconds, and a considerable streak in two minutes, while in others a very long time was required to produce any effect.

182. The dormancy of this colouring principle, under the influence of sulphurous acid, is well shown by dropping a little weak sulphuric acid on the paper bleached by that gas, which immediately restores the red colour in all its vigour. In like manner alkalies restore the colour, converting it at the same time into green.

183. *Papaver orientale.*—The chemical habitudes of the sulphurous acid render it highly probable that its action, in inducing a dormant state of the colorific principle, consists in a partial deoxidizement, unaccompanied however with disorganization of its molecules. And this view is corroborated by the similar action of alcohol already spoken of; similar, that is, in kind, though less complete in degree. Most commonly, vegetable colours, weakened by the action of alcohol, are

speedily restored on the total evaporation of that ingredient. But one remarkable instance of absolute dormancy induced by that agent, has occurred to me in the case of the *Papaver orientale*, a flower of a vivid orange colour, bordering on scarlet, the colouring matter of which is not extractable otherwise than by alcohol, and then only in a state so completely masked, as to impart no more than a faint yellowish or pinkish hue to paper, which it retains when thoroughly dry, and apparently during any length of time without perceptible increase of tint. If at any time, however, a drop of weak acid be applied to paper prepared with this tincture, a vivid scarlet colour is immediately developed, thus demonstrating the continued though latent existence of the colouring principle. On observing this, it occurred to me to inquire whether, in its dormant state, that principle still retained its susceptibility of being acted on by light, since the same powerful and delicate agent, which had been shown in so many cases as to constitute a general law, capable of disorganising and destroying vegetable colours actually developed, might easily be presumed competent to destroy the capacity for assuming colour, in such organic matter as might possess it, under the influence of their otherwise appropriate chemical stimuli. A strip of the paper was therefore exposed for an hour or two to the spectrum, but without any sensible effect, the whole surface being equally reddened by an acid. As this experiment sufficiently indicated the action of light, if any, to be very slow, I next placed a strip, partly covered, in a south-east window, where it remained from June 19 to August 19, receiving the few and scanty sunbeams which that interval of the deplorable summer of 1841 afforded. When removed, the part exposed could barely be distinguished from the part shaded, as a trifle yellower. But on applying acid, the exposed and shaded portions were at once distinguished by the assumption of a vivid red in the latter, the former remaining unchanged.

184. A mezzotinto picture was now pressed on a glazed frame over another portion of the same paper, and abandoned on the upper shelf of a green-house to whatever sun might occur from August 19 to October 19. The interval proved one of almost uninterrupted storm, rain, and darkness. On removal, no appearance whatever of any impressed picture could be discerned, nor was it even possible to tell the top of the picture from the bottom. It was then exposed in a glass jar to the fumes of muriatic acid, when, after a few minutes, the development of the dormant picture commenced, and slowly proceeded, disclosing the details in a soft and pleasing style. Being then laid by in a drawer, with free access of air,

the picture again faded, by very slow degrees, and on January 2, 1842, was found quite obliterated. Being then again subjected to the acid vapour, the colour was reproduced. How often this alternation might have gone on I cannot say, the specimen having been mislaid or destroyed. But a portion of such paper photographically impressed with a stamped pattern, accompanies this communication for the satisfaction of any member who may wish to try the experiment. The extreme slowness of the action precludes any prismatic analysis of the process, and it cannot be too often repeated *that the use of coloured glasses in such inquiries serves only to mislead.* Of dormant photographic impressions generally, whether slowly developing themselves by lapse of time, or at once revivable by stimuli, as well as of the spontaneous fading and disappearance of such impressions, I shall have more to say hereafter, having encountered several very curious cases of the kind in studying the habitudes of gold, platina, &c. I would here only observe, that a consideration of many such phænomena has led me to regard it as not impossible that the retina itself may be *photographically* impressible by strong lights, and that some at least of the phænomena of visual spectra and secondary colours may arise from the sensorial perception of actual changes in progress in the physical state of that organ itself, subsequent to the cessation of the direct stimulant.

[To be continued.]

III. *A Reply to* Professor Kelland's *Observations in the Philosophical Magazine for November* 1842. *By the Rev.* M. O'Brien *.

AS my final reply to Professor Kelland, I beg to state very briefly the following facts:—

1st. Professor Kelland has given no answer to the questions I asked him, viz. "Why did he bring forward an expression for v^2 from his 'Theory of Heat' as equivalent to mine, when it most clearly does not account for dispersion independently of the hypothesis of finite intervals?" And again, "Why did he not quote the words which follow this expression in his 'Theory of Heat,' containing his own admission that it was too uncertain to be made use of?" These questions contain my chief "charges" against Professor Kelland; and since he has not answered them, I conclude that he cannot deny his attempt to create an impression that he had, in his 'Theory of Heat,' anticipated my explanation of dispersion.

* Communicated by the Author.

2ndly. Professor Kelland now confesses that his fundamental equations are essentially erroneous. Mr. Earnshaw and myself have fully confuted his strange attempt to set up the plea of *misprint* and *mistranscription*.

3rdly. Professor Kelland does not defend his equations in page 159 of the Cambridge Transactions, vol. vi.

4thly. Professor Kelland most unquestionably denies the existence of a normal vibration in the Edinburgh Transactions, vol. xiv. page 396; and neglects taking the normal vibrations into account in that memoir, which plainly proves how little he *then* understood of the question of transversal and normal vibrations.

I now therefore am entitled confidently to assert the accuracy of all my assertions respecting Professor Kelland's investigations.

In conclusion, I beg to state that I have nowhere charged Professor Kelland with dishonesty towards M. Cauchy; and that what Professor Kelland calls "my attack upon him" is only my *defence* against his *unprovoked* attack upon me; and that when he took upon him to make a *public* attack upon me, he had no right to expect that I would have been content with a "*private explanation.*"

Dec. 2, 1842.

IV. *A Reply to* Professor Kelland's *Letter of November* 1842. *By* S. Earnshaw, *M.A.*, *Cambridge*, *and the Rev.* M. O'Brien.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

PROFESSOR Kelland in your present Number [Dec. 1842.] seeks to avoid the consequences of our arguments by stating that we have been led astray by "a misprint, or rather a mistranscription," and that the quantities we have animadverted upon "are *not* equal," and that he has "supposed the axis of y to be that along which transmission takes place." We shall reply to these in a reverse order.

1. The Professor says he has supposed the axis of y to be that of transmission.

If the Professor will refer to p. 161 of his Memoir (Camb. Phil. Trans. vol. vi.), he will find that he there states the contrary to be the case in these words: "so that we might at once suppose the direction of transmission to be the axis of y, and put δy for $\delta \rho$; this, however, *I shall not do*;" and as a proof that he did not, we find him near the bottom of the same

page writing $\delta \rho = \delta x \cos \theta + \delta y \cos \phi + \delta z \cos \psi$, which of course he would not have done had $\delta \rho$ been equal to δy. At page 166 he gives $\delta \rho$ the value δx, that is, he supposes *the axis of x* to be the axis of transmission; and a little below he gives $\delta \rho$ the value δz, that is, he supposes *the axis of z* that of transmission; thus deducing the corresponding results from his previous investigation in a manner which can only be justified by the supposition that the axis of y was not the axis of transmission. At page 179 he for the *first* time really does suppose the axis of y to be that of transmission; and this hypothesis is introduced exactly as a person would introduce it who had all along supposed his previous investigations independent of the hypothesis; his words are, "suppose, then, to *fix ideas* that the wave is transmitted along the axis of y." Now if this had been the hypothesis through the previous part of the paper, why was it here necessary to state it for the first time in order to fix ideas? The ideas would have been fixed upon it from the first. Is it not clear from this sentence, after what is said above, that the Professor did not suppose the axis of y to be the axis of transmission in the early part of his paper, to which only our remarks were directed?

2. The Professor says that the quantities we have quoted are not all equal to n^2, but that the second of them is equal to $n_{\prime}^2$.

It is at least very singular that no such quantity as $n_{\prime}^2$ is mentioned or alluded to, or its existence implied in the whole memoir; which is almost inconceivable if the Professor's MS. had contained it. Now it so happens that the integrals of the three differential equations under discussion are written down in the middle of page 163, and those integrals are correct, only on the supposition that the coefficients *are all equal*; unless indeed we suppose that the transcriber and printer have made another mistake. In lines 2 and 13 the author twice informs us what is the value of n^2, the information being necessary in order to make his readers understand the meaning of the integrals, but he makes no allusion to an $n_{\prime}^2$, though, had there been such a quantity, information of its value was as necessary as in the case of n^2. Line 4 of the same page is irreconcilable with line 2, unless the author had supposed the coefficients all equal. And, had they been unequal, there would have been *two* velocities of transmission, a circumstance which it would have been absolutely necessary to notice to prevent confusion of ideas, when the author at pages 164, 165, 167, ... speaks of *the* velocity of transmission.

3. In the places we have quoted, and some others, the equality of the coefficients is implied in such a manner that

no change short of a total alteration of the whole memoir can correct it. For instance, the discussion of the particular case beginning at the bottom of page 158, which is made the foundation of the treatment of the general case, is inapplicable except on the supposition of equality of coefficients, for the author effects his reductions on that supposition, and that the integrals are

$$\alpha = a \cos(nt - \rho), \ \beta = b \cos(nt - \rho), \ \gamma = c \cos(nt - \rho);$$

and the transference of these, unaltered, into page 163 as the proper integrals for the general case, implies beyond the possibility of a doubt that the author believed the coefficients to be equal in the general case as in the particular case. It is hard to understand, after this statement, how we, or any other person reading the Professor's Memoir, could have conjectured, that in supposing the coefficients equal, we had been led astray by "a misprint or a mistranscription."

It is clear, the *arguments* against what we have advanced in our previous communications, are exhausted; we shall therefore consider this letter as concluding our correspondence on the subject.

We are, Gentlemen,

Your obedient Servants,

S. Earnshaw.

Cambridge, Dec. 2, 1842. M. O'Brien.

P.S. Dec. 3, 1842.—Perhaps we ought to have noticed the Professor's statement, that he has proved in his Memoir (p. 180) that the three quantities are *unequal*, on the *equality* of which our arguments, in your Magazine for November, entirely depend. The Professor certainly deceives himself in thinking that he has *proved* them unequal; for what he *has* proved is simply that $v^2 + v'^2 + v''^2 = 0$; the only *legitimate* inference from which equation is, that $v = 0$, $v' = 0$, $v'' = 0$, which is precisely what we have proved from the Professor's equations of motion. But the Professor, instead of drawing this inference, has imagined from it that v' is impossible, an inference he was not *at liberty* to make, because it *violates the hypothesis* on which he had effected the reductions and transformations in the former part of his paper, upon the truth of which the correctness of the equation $v^2 + v'^2 + v''^2 = 0$ depends. If v' is impossible the integral of his second equation of motion ought to have been an exponential; and then the case considered at pages 158, 159, is dissevered from the general case; and so the whole memoir would require to be remodelled.

V. *On the Foci and Directrices of the Line of the Second Order.* By T. S. Davies, *F.R.S. Lond. and Ed., F.S.A., &c., Royal Military Academy.*

IN vol. viii. (pp. 317–21.) of Gergonne's *Annales des Mathématiques*, M. Bret has proposed a method of discovering the foci of a conic section, defined by the usual general equation of the second degree between x and y. He adopts as his definition of the focus, that of Euler: the focus is a point in the plane of the curve, such that its distance from any point in the curve is a linear function of the corresponding coordinates of the curve. The investigation, however, is not carried beyond rectangular coordinates, and he does not solve his resulting equations except for two very simple cases. Those equations are certainly, to the eye, very simple; but on attempting their solution, it will be found that they involve an amount of reduction such as we should be little prepared to expect. Moreover, the conclusion he draws from his equation (that there are *four* foci, two real and two imaginary) does not seem to be justified by the investigation about to be here offered.

Sir John Lubbock has also investigated, under a view which is virtually the same as Euler's, but by a very elegant process peculiar to himself, the existence of a focus in the line of the second order*. As the subject is introduced incidentally and briefly in Sir John's paper, there is nothing to lead us to any conclusion as to how far he had pushed these researches.

These two are all the discussions of the problem of the foci with which I am acquainted, though possibly others might be scattered in some of the continental periodicals which I have no means of consulting. As the method which I have employed is perfectly general, and I have completely resolved the resulting equations, giving at the same time both the foci and directrices, instead of the foci alone, I am disposed to think it will not be without interest to those geometers who have been in the habit of studying the conic sections by means of their coordinate equations. One or two applications of the formulæ here obtained may be given in a future Magazine.

Theorem.

A point and a line can be found in the plane of a line of the second order, referred to any system of rectilinear coordinates, such that any point in the line of the second order shall have its distances from the point and line to be found in a constant ratio, which can also be found.

* Phil. Mag. S. 3. vol. x. Aug. 1831, p. 86.

First. The Ellipse and Hyperbola.

Denote the equations of the given curve and of the line to be found, respectively, by

$$a y^2 + b x y + c x^2 + d y + e x + f = 0 \quad . \quad . \quad (1.)$$

$$p y + q x + r = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

Also, let $(h\, k)$ be the point to be found, m the constant ratio, and α the given angle of ordination.

Retaining the same angular directions of the coordinate-axes, transfer the origin to the centre of the curve; then the preceding equations become

$$a y^2 + b x y + c x^2 + f' = 0 \quad . \quad . \quad . \quad (3.)$$

$$p y + q x + r' = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

$$\text{where } r' = \frac{2 c d - b e}{b^2 - 4 a c} p + \frac{2 a e - b d}{b^2 - 4 a c} q + r \quad . \quad . \quad . \quad (5.)$$

$$f' = \frac{a e^2 + c d^2 - b d e}{b^2 - 4 a c} + f \quad . \quad . \quad . \quad . \quad (6.)$$

If now D and P represent the respective distances of a point in the curve from the point and line to be found, the proposition affirms that $D = m P$, or $D^2 = m^2 P^2$. Also, if $h'\, k'$ be the point in reference to the new axes of coordinates, this condition will be expressed by

$$\left.\begin{array}{l} (x - h')^2 + 2 (x - h') (y - k') \cos\alpha + (y - k')^2 \\ \qquad = \dfrac{m^2 (p y + q x + r)^2 \sin^2\alpha}{p^2 - 2 p q \cos\alpha + q^2} \quad . \quad . \end{array}\right\} \quad . \quad . \quad (7.)$$

It will be convenient, before proceeding further, to lay down the following notation. The functions, indeed, from their perpetual occurrence in researches concerning the conic sections, ought to be designated in some uniform manner. Those here employed, from their not interfering with any of the notations generally used in this class of inquiries, may perhaps be found worthy of adoption.

$$\left.\begin{array}{l} u^2 = p^2 - 2 p q \cos\alpha + q^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \\ R^2 = (a - b \cos\alpha + c)^2 + (b^2 - 4 a c) \sin z, \quad . \\ Q = a - b \cos\alpha + c, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \\ H = Q - 2 a \sin^2\alpha = a \cos 2\alpha - b \cos\alpha + c, \\ K = Q - 2 c \sin^2\alpha = a - b \cos\alpha + c \cos 2\alpha, \end{array}\right\} \quad . \quad (8.)$$

Expand (7.) and identify the result with (3.) by equating the homologous coefficients; then we have

$$a = u^2 - m^2 p^2 \sin^2\alpha \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

$$b = 2 u^2 \cos\alpha - 2 m^2 p q \sin^2\alpha \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10.)$$

$$c = u^2 - m^2 q^2 \sin^2 \alpha \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11.)$$

$$0 = u^2 (k' + h' \cos \alpha) + m^2 p r' \sin^2 \alpha \quad . \quad . \quad . \quad . \quad . \quad . \quad (12.)$$

$$0 = u^2 (h' + k' \cos \alpha) + m^2 q r' \sin^2 \alpha \quad . \quad . \quad . \quad . \quad . \quad . \quad (13.)$$

$$f = u^2 (h'^2 + 2 h' k' \cos \alpha + k'^2) - m^2 r'^2 \sin^2 \alpha \quad . \quad . \quad (14.)$$

The quantities h', k', p, q, r', m being determined from these equations, the proposition will be established.

Multiply (10.) by $\cos \alpha$ and subtract the result from the sum of (9.) and (11.); then

$$a - b \cos \alpha + c = 2 u^2 \sin^2 \alpha - m^2 \sin^2 \alpha \, (p r - 2 p q \cos \alpha + q^2), \text{ or}$$

$$Q = (2 - m^2) u^2 \sin^2 \alpha \quad . \quad . \quad . \quad . \quad (15.)$$

Again, from the same three equations, we have

$$m^2 p^2 \sin^2 \alpha = u^2 - a \quad . \quad . \quad . \quad . \quad . \quad . \quad (16.)$$

$$2 m^2 p q \sin^2 \alpha = 2 u^2 \cos \alpha - b \quad . \quad . \quad . \quad (17.)$$

$$m^2 q^2 \sin^2 \alpha = u^2 - c \quad . \quad . \quad . \quad . \quad . \quad . \quad (18.)$$

From these, since

$$(2 m^2 p q \sin^2 \alpha)^2 = 4 (m^2 p^2 \sin^2 \alpha)(m^2 q^2 \sin^2 \alpha), \text{ we get}$$

$$(2 u^2 \cos \alpha - b)^2 = 4 (u^2 - a)(u^2 - c), \text{ or}$$

$$4 u^4 \sin^2 \alpha - 4 (a - b \cos \alpha + c) u^2 = b^2 - 4 a c; \text{ or again,}$$

$$2 u^2 \sin^2 \alpha = Q + R, \text{ and } u^2 = \frac{Q + R}{2 \sin^2 \alpha} \quad . \quad . \quad . \quad . \quad (19.)$$

Resolve (15.) for m^2, and insert (19.) in the result; then

$$m^2 = \frac{2 R}{Q + R} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20.)$$

the constant ratio, or "determining ratio" is hence found.

For the determination of p and q, substitute the values of m^2 and u^2 from (19, 20.) in (16, 18.); then after very slight reduction we obtain

$$p^2 = \frac{u^2 - a}{m^2 \sin^2 \alpha} = \frac{(Q - 2 a \sin^2 \alpha + R)(Q + R)}{4 R \sin^4 \alpha} = \frac{(H + R)(Q + R)}{4 R \sin^4 \alpha} \quad (21.)$$

$$q^2 = \frac{u^2 - c}{m^2 \sin^2 \alpha} = \frac{(Q - 2 c \sin^2 \alpha + R)(Q + R)}{4 R \sin^4 \alpha} = \frac{(K + R)(Q + R)}{4 R \sin^4 \alpha} \quad (22.)$$

The signs of p and q deduced from these are double, and they are in each case to be so taken (like or unlike) as to fulfil (17.), viz. to be alike when $2 u^2 \cos \alpha - b$, or $(a + c + R) \cos \alpha - b \cos 2 a$ is positive, and unlike when this quantity is negative. In either case the two values express sameness of inclination to either axis of coordinates, and hence parallelism of position. Wherefore in the ellipse and hyperbola there are two such lines as that of which the proposition affirms the existence.

It would also be easy to show that these lines are perpen-

dicular to one of the conjugate rectangular axes, did space allow us to dilate.

We shall now proceed to the other three equations (12, 13, 14.) which, together with m, p, q already found, involve h', k', r'.

From (12, 13.) resolved for h' and k', we get

$$h' = -\frac{m^2 r' (q - p \cos \alpha)}{u^2} \quad . \quad . \quad . \quad . \quad (23.)$$

$$k' = -\frac{m^2 r' (p - q \cos \alpha)}{u^2} \quad . \quad . \quad . \quad . \quad (24.)$$

Also, multiply (12.) by k' and (13.) by h', and add; then

$$u^2 (h'^2 + 2 h' k' \cos \alpha + k'^2) = - m^2 r' (p k' + q h') \sin^2 \alpha \ . \ (25.)$$

Insert (23, 24.) in (25.); whence there results on reduction,

$$u^2 (h'^2 + 2 h' k' \cos \alpha + k'^2) = m^4 r'^2 \sin^2 \alpha \quad . \quad . \ (26.)$$

Write for the left side of this equation its value on the right in (14.); then we immediately obtain by means of (20.),

$$r'^2 = \frac{f'}{m^2 (m^2 - 1) \sin^2 \alpha} = \frac{(\mathrm{R} + \mathrm{Q})^2 f'}{2 \mathrm{R} (\mathrm{R} - \mathrm{Q}) \sin^2 \alpha} \quad . \ (27.)$$

This gives two values of r', and since they are equal, the two lines to be found are symmetrically situated with respect to the centre.

Again, substitute the values of r', p, q, u^2 in (23, 24.), and we obtain the values of h' and k'; viz.

$$h' = \pm \sqrt{\frac{\frac{1}{2} f'}{b^2 - 4 a c}} \cdot \frac{\sqrt{\mathrm{H} + \mathrm{R}} \cdot \cos \alpha - \sqrt{\mathrm{K} + \mathrm{R}}}{\sin^2 \alpha} \ . \ (28.)$$

$$k' = \pm \sqrt{\frac{\frac{1}{2} f'}{b^2 - 4 a c}} \cdot \frac{\sqrt{\mathrm{K} + \mathrm{R}} \cos \alpha - \sqrt{\mathrm{H} + \mathrm{R}}}{\sin^2 \alpha} \quad . \ (29.)$$

These show that the points to be found are also symmetrically situated with respect to the centre, and with respect to the axes of coordinates. It also appears from these equations that h' and k' are always of *different signs*. Also, since

$$h = h' + \frac{2 a c - b d}{b^2 - 4 a c} \text{ and } k = k' + \frac{2 c d - b e}{b^2 - 4 a c},$$

we get by merely substituting the value of $\frac{1}{2} f'$ from (6.), the value of r from (5, 27.), and those of p, q from (21, 22.). The complete solution of the system of equations, and therefore the demonstration of the theorem, is effected.

With respect to the conditions of reality of these expressions, little need be said here, as the subject is discussed in several treatises on lines of the second order with sufficient amplification, the functions here involved occurring in almost all inquiries relating to these curves.

Throughout I have written R with the positive sign; but this is done to avoid the interference of the double sign derived from this source with others that occur in the investigation: but the change is easily made for the other case from the forms here given.

When Q is absolutely greater than R, or $(a - b\cos\alpha + c)^2$ greater than $(a - b\cos\alpha + c)^2 + (b^2 - 4ac)\sin^2\alpha$; or again, $b^2 - 4ac$ negative, the curve (3.) is the ellipse referred to its centre, and f' is known in this case to be *essentially negative.* Wherefore R must have the sign that will render H + R and K + R both positive; that is, R is to be taken positive.

When, on the contrary, $b^2 - 4ac$ is positive, or the curve is the hyperbola, f' may be either negative or positive, according as the hyperbola is the primary or the conjugate one. These two cases will require that R shall be respectively negative or positive.

Second. The Parabola.

The preliminary transformation of coordinates cannot in this case be made in the same manner as in the preceding one. I have therefore deduced the truth of the theorem without the aid of any transformation. However, as the expressions, though greatly simplified in the parabola by the relation $b^2 - 4ac = 0$, are not so simple as when the origin is transposed to a point $x_, y_,$ in the curve, I shall here give the investigation according to this latter method. The general investigation might, indeed, be conducted in an analogous manner; but for the ellipse and hyperbola this would have led to expressions much more complex than we have had occasion to use in the method already developed.

The equations to be resolved become, under this transformation,

$$a = u^2 - m^2 p^2 \sin^2\alpha \quad \ldots \quad (1.)$$

$$b = 2u^2\cos\alpha - 2m^2 pq\sin^2\alpha \quad \ldots \quad (2.)$$

$$c = u^2 - m^2 q^2 \sin^2\alpha \quad \ldots \quad (3.)$$

$$d' = -2u^2(k' + h'\cos\alpha) - 2m^2 p r' \sin^2\alpha \quad . \quad (4.)$$

$$e' = -2u^2(h' + k'\cos\alpha) - 2m^2 q r' \sin^2\alpha \quad . \quad (5.)$$

$$0 = u^2(h'^2 + 2h'k'\cos\alpha + k'^2) - m^2 r^2 \sin^2\alpha \quad . \quad (6.)$$

where
$$\left.\begin{aligned} d' &= 2ay_, + bx_, + d \\ e' &= 2cx_, + by_, + e \end{aligned}\right| \quad \left.\begin{aligned} h' &= h - x_, \\ k' &= k - y_, \end{aligned}\right\} \quad . \quad (7.)$$

From (1, 2.) and (2, 3.) we get, as in the former case,

$$u^2 = \frac{Q}{\sin^2\alpha} \quad \ldots \quad (8.)$$

$$m^2 = \frac{2R}{Q + R} = 1 \quad \ldots \quad (9.)$$

since here Q = R.

Again, from (1, 3, 8, 9.) we have, after slight reduction by means of the relation $b = 2\sqrt{a\,c}$, the equations

$$p = \frac{\sqrt{a}\,.\cos\alpha - \sqrt{c}}{\sin^2\alpha} \quad . \quad . \quad . \quad . \quad . \quad (10.)$$

$$q = -\frac{\sqrt{c}\,.\cos\alpha - \sqrt{a}}{\sin^2\alpha} \quad . \quad . \quad . \quad . \quad (11.)$$

the sameness or difference of the signs of which are to be determined by the relation (2.).

From (4, 5.) and (9.), we have

$$2\,u^2\,(k' + h'\cos\alpha) = -\,(d' + 2\,p\,r'\sin^2\alpha) \quad . \quad . \quad . \quad (12.)$$

$$2\,u^2\,(h' + k'\cos\alpha) = -\,(e' + 2\,q\,r'\sin^2\alpha) \quad . \quad . \quad . \quad (13.)$$

Resolving which for h', k', and putting Q for $u^2\sin^2\alpha$,

$$h' = \frac{d' - e' + 2\,(p\cos\alpha - q)\,r'\sin^2\alpha}{2\,Q} \quad . \quad . \quad . \quad (14.)$$

$$k' = \frac{e' - d' + 2\,(q\cos\alpha - p)\,r'\sin^2\alpha}{2\,Q} \quad . \quad . \quad . \quad (15.)$$

Now we have from (10, 11.),

$$p\cos\alpha - q = \frac{\sqrt{a}.\cos^2\alpha - \sqrt{c}.\cos\alpha + \sqrt{c}.\cos\alpha - \sqrt{a}}{\sin^2\alpha} = -\sqrt{a} \;(16.)$$

$$q\cos\alpha - p = -\frac{\sqrt{c}.\cos^2\alpha - \sqrt{a}.\cos\alpha + \sqrt{a}.\cos\alpha - \sqrt{c}}{\sin^2\alpha} = +\sqrt{c} \;(17).$$

Insert (16.) in (14.), and (17.) in (15.); then we get

$$h' = \frac{d' - e' - 2\sqrt{a}\,.\,r'\sin^2\alpha}{2\,Q} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18.)$$

$$k' = -\frac{d' - e' - 2\sqrt{c}\,.\,r'\sin^2\alpha}{2\,Q} \quad . \quad . \quad . \quad . \quad . \quad (19.)$$

Express by means of (18, 19.) the value of $h'^2 + 2\,h'\,k'\cos\alpha + k$, and insert in (6.); then after very slight reduction,

$$r' = \frac{d' - e'}{2\,(\sqrt{a} + \sqrt{c})\sin^2\alpha} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20.)$$

Insert the value of $2\,r'\sin^2\alpha$ from (20.) in (18, 19.), and there results

$$h' = \frac{(d' - e')\sqrt{c}}{2\,Q\,(\sqrt{a} + \sqrt{c})} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21.)$$

$$k' = \frac{(d' - e')\sqrt{a}}{2\,Q\,(\sqrt{a} + \sqrt{c})} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22.)$$

We have hence only to correct these results for the coordi-

nates of transformation, to obtain the several values required by the proposition.

It may be remarked that the coordinates $x_{\prime}y_{\prime}$ are only connected by a single equation, that of the parabola; and hence another condition might be introduced, some one perhaps which would usefully simplify the results. Several such may be suggested; as for instance, $x_{\prime}\, y_{\prime}$ to be such a point in the curve as should render P a minimum: but this is not the proper place for such details.

Postscript.

I take this opportunity (the earliest and the most proper one that has occurred) of correcting a mistake into which Professor De Morgan has fallen with respect to the authorship of a paper on the works of Alexander Anderson, which was published in the 'Ladies' Diary' for 1840 (Companion to the Almanac, 1843, p. 9.). That paper was written by my late friend Dr. Gregory, and not by me, as stated in the place referred to. It was, I am pretty sure, the very last dissertation he ever published. I am the more anxious to disclaim it early, as the authority of Mr. De Morgan would otherwise render it a matter of history, and I am by no means desirous to have the works of other geometers, however valuable, ascribed to me.

Whilst I am on this subject, I have to correct a mis-statement of my own in the Philosophical Magazine for August last. I had attributed the theorem of which I there gave a demonstration to Dr. Wallace, mainly on his own authority. I have since found that the same theorem had been given about thirty years earlier by Lambert (in his *Insigniores orbitæ cometarum proprietates*, section i.), together with the other theorems published in the 'Mathematical Repository'. (Poncelet, *Traité de Propriétés Projectives*, p. 268.).

It may also be worth noticing, that amongst the foreign proofs of Pascal's Hexagram, I have found since my own was printed (Phil. Mag., July 1842.), two very elegant ones, which deserve to be known in this country; one by Magnus in Crelle's Journal, which is copied into the 'Ladies' Diary' for 1843; and the other by Levy in *tom.* iv. Quetelet's Correspondence, p. 24. Five others also are given in the 'Diary' by English writers.

Royal Military Academy, Dec. 3, 1842.

VI. *Letter to* R. Phillips, *Esq., F.R.SS. L. and E., on the subject of* Professor Daniell's *last Communication. By* W. R. Grove, *Esq., M.A., F.R.S., Professor of Experimental Philosophy in the London Institution.*

My dear Sir,

I HAD hesitated (for reasons with which I shall not trouble you) in replying to Professor Daniell's last letter, but on consideration I have determined to make a few comments upon it.

It appears that the assertion of Professor Daniell, "that I had never spoken of my battery but as the further *application* of principles which he had previously deduced, "proceeded from his supposing he had heard me at the London Institution "admit, that it was in following up his train of reasoning I was led to the construction of my battery."

On the occasion mentioned I showed the gold leaf experiment; I have now in my laboratory the identical gold leaf, prepared by my then assistant, Mr. Styles; it is gummed on glass and the one half dissolved.

I did in that lecture give Mr. Daniell all the credit I could for the invention of his battery, and avoided, as much as possible, any allusion to points on which I differed from him. I thought then, and I think now, that I should have been guilty of singularly bad taste had I done otherwise; I then believed Mr. Daniell's presence an act of courtesy, I felt grateful for it and so expressed myself; I frankly admit that I did pay him "a greater compliment than the occasion required:" I hope, if Professor Daniell or any other gentleman should feel anxious to publish, fifteen months after date, what I said at an extempore lecture, they will have the kindness first to ask me whether or not I was rightly understood. Although I certainly did not think it necessary at my lecture to state pointedly wherein I differed from him, yet the tone of Professor Daniell's letter obliges me now, in order to obviate further mistakes, to review some of the passages of his and my *writings* on this subject.

Professor Daniell, Phil.Mag., Dec. 1842, p. 421.

"....the nitric acid battery *exactly* resembles the constant battery in *every* particular *except* the substitution of platinum and nitric acid for copper and sulphate of copper; and an experimentalist might, *very obviously* [!], have been led to

Professor Daniell, Phil. Trans., 1836, p. 119.

"Upon adding *nitric acid* to the solution of sulphate of copper, I found that an *injurious* effect was produced; and that the mean quantity of gas in five minutes was *lowered* to 1·1 cubic inch; at this rate of action, the battery, however,

the change by following up the *principle* of diminishing contrary electromotive powers and resistances to a current originating with the zinc."

remained steady for six hours."

The "very obviously" requires no comment; Mr. Daniell will, I have no doubt, have the goodness to point out to me where *he* has published anything about the principle of contrary electromotive powers and resistance previously to the publication of my battery; I cannot find it, and yet I suppose this is the principle which he deduced and I further applied. Although I have stated the actual deduction which led to my battery, I have no right to disclaim any assistance which I *might* have received from experiments published previously to mine; I object to no one for referring my experiments to any previous ones; I only protest, and intended my last letter on this subject as a *temperate* protest, against being represented as myself assenting to certain undefined principles. One more parallel passage.

Professor Daniell, Phil. Mag., Dec. 1842, p. 421.

"What this [the gold leaf experiment] has to do with the nitric acid battery, in which the two acids in contact are the nitric and *sulphuric*, I really cannot perceive. The origin of the force in this case has always appeared to me to be the action of the zinc upon the dilute sulphuric acid, but Professor Grove may possibly consider it to be still the contact of the two acids. He has, however, stated that he was *so led* to the construction of his battery, &c."

Mr. Grove, Phil. Mag., May 1839, p. 388, referred to in my letter of November last.

"It now occurred to me, that as gold, platina, and two acids [the nitric and *hydrochloric*] gave so powerful an electric current, *à fortiori*, the same arrangement, with the substitution of zinc for gold, must form a combination more energetic than any yet known." [And so it did.]

I find nothing here about the origin of the force in this case being the contact of the two acids; Professor Daniell will, perhaps, be good enough also to direct my attention to where I have so expressed myself.

I find, moreover, that three elements out of four differed from those of Mr. Daniell, and that the fourth, viz. zinc, is that which has been used for voltaic batteries since the time of Volta. I subsequently recommended sulphuric acid as a cheap substitute for hydrochloric: I will not enter into detail

on this point, it is too frivolous. New batteries, or new philosophical experiments of any sort may differ in every particular from old ones and yet be valueless, while on the other hand a very slight alteration may constitute an important discovery. Mr. Daniell's battery varied only in *one* element from those previously known.

I have already published my opinions on the principles of the voltaic battery and need not repeat them, but as it is necessary to be explicit, I will refer to one other point wherein I differed, and still differ, from Professor Daniell. In the above-mentioned lecture at the London Institution I drew a broad line (not an imaginary line, but a broad line with chalk on a black board) between metallic solutions and highly oxygenated acids, such as the nitric, chloric, &c.; I still hold to this distinction. Now Professor Daniell, in his long paper recently published containing many experiments on my battery, interspersed with simple equations, arrives in the last page at the following conclusion:—"If chloride of platinum were not too expensive to allow of its being employed as the exterior part of the electrolyte in contact with a platinum conducting plate, e', or the contrary electromotive force would be wholly annihilated, as nothing but platinum would be thrown down upon the platinum, and it would constitute the most perfect possible arrangement, but would not much, if anything, exceed the efficiency of nitric acid."—*Phil. Trans.*, 1842, p. 287.

This is no *verbal* statement, but a paper, I presume, deliberately prepared, communicated to the Royal Society, and published in its Transactions. Now I deliberately assert that the above arrangement is *not* the most perfect possible, nor nearly so; that it is very inferior to the nitric acid; that the principles of the voltaic battery, as I conceive them, led me to this conclusion, and that experiment confirmed it. I find, moreover, that I am not solitary in this opinion. I am glad that Mr. Daniell and myself here differ, not merely upon principles or their application, but upon *facts*, of the correctness of which any *experimentalist* can satisfy himself. I will venture to suggest to any one who may like to try the experiment, that care should be taken not to have any nitric acid mixed with the chloride of platinum, as if so, from its *great superiority* as an electrolyte (having five equivalents of an electro-negative element), the nitric acid will be deoxidated and the platinum *not* thrown down *.

* Note, Dec. 19.—My attention has just been called to a report of my lecture, Phil. Mag. S. 3, vol. xviii. p. 234. This confirms what I have said above, and proves that the lecture was simply an historical review of the voltaic piles used in practice and an explanation of them all on the *chemical theory*.

I write this letter, not to expose error, but to prevent *misconception*. Scientific controversy is, it is to be hoped, agreeable to no one; it is most disagreeable to me, and I have, it seems, become entangled in it by seeking to avoid it, but I will not be misconceived by the public; had I not been mentioned (and it appears to me quite unnecessarily mentioned) I should have been much better pleased to have remained silent. Professor Daniell has (probably for good reasons) avoided my name when making a copious use of my results*; I wish he had carried out this principle, and not introduced my name into a controversy with which I have nothing at all to do. I remain, my dear Sir, yours very truly,

Swansea, Dec. 1842. W. R. Grove.

VII. *The Ordnance Survey.*

It is with no small satisfaction that we announce to our astronomical friends the appearance of a new volume of the Trigonometrical Survey. During the long interval of thirty-one years which has elapsed since the publication of the last volume in 1811, the sole fruits of this important and costly operation have been a series of county maps—admirably executed, we admit, and of the highest value in reference to the topography of the country—still in course of preparation and publication. No observations or results connected with geodesy have been officially communicated; and indeed if we except the few meagre accounts which have been occasionally furnished to Parliament, the public, generally, has had no information respecting the state and progress of the work. The appearance of the present volume is therefore highly gratifying, not only on account of the observations it contains, but also by reason of the promise held out that the mass of valuable results which has so long been accumulating will at length be available for the better determination of the dimensions and figure of the earth. After all, it may ultimately be found that the delay which has taken place in completing the meridional arc is not greatly to be regretted. Within the last twenty years the theory of computing geodetical observations has received considerable improvements in the hands of Gauss and Bessel, and advantage will no doubt be taken of the new methods to render the results as perfect as possible. The instruments which have been used in the Ordnance Survey, both for the astronomical and geodetical observations, have been far superior to those which were employed in the celebrated operation for determining the French arc of meridian,

* See Phil. Trans. 1842, p. 271 *et passim.*

and indeed in any of the continental surveys; and it is very important that none of the advantages of this superiority be lost through the use of imperfect, or rather, not the most perfect, methods of reduction and computation.

The contents of the present volume are sufficiently indicated by its title*. They include all the observations for purposes connected with the survey made with Ramsden's zenith sector, the *chef d'œuvre* of that celebrated artist. The number of stations at which observations were made is ten, namely, Dunnose, Dunkirk, Greenwich (two series), Arbury Hill, Delamere, Clifton Beacon, Burleigh Moor, Kellie Law in Fifeshire, Cowhythe Hill in Banffshire, and Balta, the easternmost of the Shetland Islands. Those at Dunnose, Greenwich (first series), Arbury Hill, and Clifton Beacon were made in 1802, and *partly* published in Mudge's "Account of the measurement of an arc of the meridian extending from Dunnose in the Isle of Wight to Clifton in Yorkshire," printed in the Philosophical Transactions for 1803, and in the second volume of the Trigonometrical Survey in 1804. Those at Delamere Forest and Burleigh Moor were made in 1806, and partly published in the third volume of Survey in 1811. Of the remaining observations, which now appear for the first time, those at Kellie Law and Cowhythe Hill were made in 1813, those at Balta in 1817, those at Dunkirk in 1818, and those at Greenwich (second series) in 1836.

The reason assigned by Colonel Colby for the republication of the observations is the following: "As the observations which had been published by the late Major-General Mudge in 1802 (1803) and 1811 *formed but a very small proportion of the whole body of observations*, most of which remained unpublished, and partial differences would result from the adoption of more rigid astronomical reductions, I deemed it most expedient to republish those observations in this volume, and thus to furnish a congruous work for future reference."

Although the practice of selecting a portion of the observations, which we are thus informed was adopted in the early part of the survey, is one which cannot be too strongly discommended, it is proper to remark that the changes which have now been made in the amplitudes in consequence of the insertion of the observations which were omitted, the correction of some errors of calculation, and the use of different ele-

* Astronomical Observations made with Ramsden's Zenith Sector, together with a Catalogue of the Stars which have been observed, and the Amplitudes of the celestial Arcs deduced from the Observations at the different Stations. Published by order of the Board of Ordnance: printed by Palmer and Clayton, Crane Court, Fleet Street, and sold by Mr. J. Arrowsmith, 10, Soho Square, and Messrs. Letts and Son, 8, Cornhill, the Agents for the sale of the Ordnance Maps. 1842.

ments in reducing the places of the stars, are extremely small, the greatest, that between Dunnose and Burleigh Moor, amounting only to 0″·65, while that between Dunnose and Clifton amounts only to 0″·04.

In the present volume the amplitudes of the several arcs are first deduced from the observations of each star separately, and the most probable mean found by assigning to each partial result the weight due to the number of observations. The final results are contained in the following table: and it is only necessary to remark that the latitudes are obtained by adding consecutively the amplitudes deduced from the sector observations to the latitude of Greenwich (51° 28′ 38″·3) as determined by the Astronomer Royal. The former results are added for the sake of comparison.

Station.	Latitude.	Amplitude.	As published in the volumes of the Trigonometrical Survey.	
			Latitude.	Amplitude.
	° ′ ″	° ′ ″	° ′ ″	° ′ ″
Dunnose	50 37 7·03		50 37 8·60	
Dunkirk.........	51 1 57·93	0 24 50·90		
Greenwich (1)	51 28 38·30	0 51 31·27	51 28 40·00	0 51 31·39
Greenwich (2)	51 28 38·79	0 51 31·76		
Arbury Hill ...	52 13 27·14	1 36 20·11	52 13 28·58	1 36 19·98
Delamere	53 13 18·77	2 36 11·74	53 13 20·80	2 36 12·20
Clifton Beacon	53 27 30·45	2 50 23·42	53 27 31·59	2 50 23·38
Burleigh Moor	54 34 19·48	3 57 12·45	54 34 21·70	3 57 13·10
Kellie Law......	56 14 50·51	5 37 43·48		
Cowhythe	57 41 9·74	7 4 2·71		
Balta	60 45 2·31	10 7 55·28		

The difference, 0″·49, between the latitudes of Greenwich (1) and Greenwich (2), is owing to the circumstance that the position of the instrument in 1802 was fifty feet south of its position in 1836.

It will be observed that the amplitude of the whole arc from Dunnose to Balta is 10° 7′ 55″·28. In point of extent this is the *second* arc which has been measured in Europe; but when we compare the instrument with which the astronomical amplitude was determined with that which was used for the French arc, we can have no hesitation in regarding it as undoubtedly the first in respect of probable accuracy. The amplitude of the French arc of meridian, between the parallels of Formentera and Dunkirk, is 12° 22′ 12″·74; and that of the Russian arc measured by Struve and Von Tenner, 8° 2′ 28″·91. Balta, we believe, is situated about 52′ west of the meridian of Greenwich, and consequently only about 20′ east of the meridian of Dunnose.

The superb instrument with which the above determinations were made, having been deposited for safety in the long Armoury at the Tower, was most unfortunately destroyed by the fire which consumed that part of the edifice in 1841.

The volume concludes with the announcement that the triangulation for connecting all the stations by geodetical distances is in an advanced state, and will be given in a subsequent publication.

VIII. *On the Evidence of a general Whirling Action in the Providence Tornado. By* W. C. Redfield, *A.M.**

ON the 30th of August, 1838, between the hours of 3 and 4 p.m., a violent whirlwind or tornado visited the town of Providence, in the State of Rhode Island. It was preceded by a shower of rain of short duration, after which the tornado appeared, appended to another cloud, and passed through the southern part of the town nearly from west to east.

Its earliest ravages reported, were in Johnston, at the farm of Mr. Randall, about seven miles west from Providence. From this point it passed on through Cranston and Providence, where, crossing the river into the State of Massachusetts, it passed through Seekonk, Rehoboth, Swansey, Somerset, and as far, at least, as Freetown, beyond Taunton river; a distance of twenty-five miles from the point first mentioned.

The width of its visible track, as indicated by the prostration of trees, fences, and other objects, varied from a mere trace in its narrowest, to two hundred yards or upwards in its widest portions. Having a few days after the occurrence of the tornado carefully examined the track for the distance of about seven miles, on each side of Providence river, I propose to offer some of the results of this examination, together with such remarks as may seem justly deducible from the effects observed.

So far, however, as the impressions made on an accidental eye-witness of the tornado may be important, we have a valuable account furnished us in the letter of Zachariah Allen, Esq., of Providence, which is given in Dr. Hare's notice of this tornado. [This (Silliman's) Journal, vol. xxxviii. p. 74–77.] Mr. Allen had the advantage of viewing its progress from a point near its path. He calls it a "whirlwind," and describes its phænomena in a manner perfectly consistent with this appellation. "The circle formed by the tornado" on the river, he describes as "about three hundred feet in diameter,"

* From Silliman's American Journal of Science and Arts.

and mentions, that the "misty vapours" ... "entering the whirlwind vortex, at times veiled from sight the centre of the circle, and the lower extremity of the overhanging cone of dark vapour;" and that "amid all the agitation of the water and the air about it, this cone continued unbroken," &c.

This "cone" of the tornado of which he so often speaks, it should be noted was an *inverted* one, the smaller end of which was sweeping on the earth's surface*. Thus he gives the instance, "when the point of the dark cone of cloud passed over the prostrate wreck of the building, the fragments seemed to be upheaved," &c. It will be seen here that the *prostration* of the building had preceded the arrival of the centre or "point" of the "cone;" showing that the whirlwind often acts on a large area, with great force, *externally* to the lower part of the *visible* cone, or the column of vapour at its axis. Moreover, the substances which by the centre of the tornado were "uplifted high in the air," were "left to fall from the OUTER EDGE of the black conical cloud†."

* We may properly conceive of this "cone," in tornadoes or water-spouts, as including not only the visible clouded condensation here described, but also the invisible portion of the whirlwind which surrounds the narrow and depending portion of the visible cone, below the general line of condensation. Thus the *entire* body of the whirlwind is generally a truncated cone; its smaller and most active end sweeping along the surface of the earth or sea.

† Mr. Allen states that the form of the cloud and of the cone of vapour depending from it so nearly resembled the engraved pictures of 'water-spouts' above the ocean, that he should have come speedily to the conclusion that one of these 'water-spouts' was approaching, had he not been aware that "this phænomenon occupied a space in the heavens directly above a dry plain of land." Perhaps it might be inferred that Mr. Allen had partaken of the too common notion, that the misnamed *water-spout* is, or should be, literally a *spout of water*. This phænomenon, so much talked of among mariners, proves to be nothing more nor less than the visible inverted "tapering cone of vapour" or condensation, noticed by him as "extending from the cloud to the surface of the earth," *at the axis or ascending portion of the whirl*; if we may at all rely on the results of extensive examinations and comparisons of the accounts of 'water-spouts' and their effects. The same appearance was observed in the New Brunswick tornado by experienced seamen navigating the Raritan river, who at once pronounced it to be a water-spout, and took their measures accordingly. It is probable, however, that most of the 'water-spouts' noticed at sea, are inferior in size and energy to these destructive tornadoes.

A 'water-spout' was seen by Messrs. Tyerman and Bennett near Borabora in the Pacific, which extended nearly horizontally from one cloud to another directly over their heads; and no harm done! The most credulous will hardly conceive this to have been *a column of water*, or even approximately such: besides, no sea-water has ever been known to fall from the clouds. Similar 'spouts' have been seen by others; and I once beheld a magnificent example of this kind in one of the interior towns of Connecticut, which probably indicated an axis of rotation nearly horizontal.

Mr. Allen says further, "The progress of the tornado was nearly in a straight line, following the direction of the wind, with a velocity of perhaps eight or ten miles per hour. Near as I was to the exterior edge of the circle of the tornado, I felt no extraordinary gust of wind, but noticed that the breeze continued to blow uninterruptedly from the same quarter from which it prevailed before the tornado occurred. I also particularly observed that there was no perceptible increase of temperature of the air adjacent to the edge of the whirlwind, which might have caused an ascending current by a rarefaction of a portion of the atmosphere."

Soliciting a careful attention to the observations of Mr. Allen, who is well known for his intelligence and his habits of correct observation, I proceed to give some account of my own examinations of the traces of this tornado.

From a point on the rocky "ledge" north of the turnpike road and nearly three miles westerly from Providence, to the house of John Burr on the Cranston road, a distance of about one and a quarter mile, I found the course of the tornado to have been S. 86° E. by compass, over a plain country. The magnetic variation being here about 8° westerly, makes the true course E. 3° N. From this point to Providence river, a distance of about two miles, the course was five degrees more northerly.

I agree with Dr. Hare that the general effects observed on this track were "quite similar" to those of the New Brunswick tornado; and will give such of my sketches, formerly prepared, as will best illustrate this similarity and the general effects here mentioned.

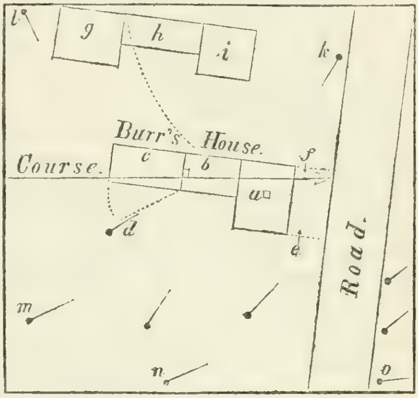
The following is a sketch of some of the effects on the farm of Mr. Burr: his house is about one mile and a half from the Providence bridge.

The effects here exhibited appear to me to be due to a progressive whirlwind, revolving to the left; for we may notice, as in the New Brunswick tornado, a more onward direction in the trees prostrated on the *right* of the axis, *d*, *m*, *n*, *o*, &c., than on the left side; while the outermost prostrations on the right, *n*, *o*, point still more nearly than the average on this side, to the course of the tornado: and on the left side of the track we have the tree *k* in a direction inclined several degrees *backward* from the course of the storm. The value of these indications of whirling action I have endeavoured to point out in my remarks on the New Brunswick case. [Silliman's Journal, vol. xli. p. 70–75.]

At the front of the house *a*, however, were two slatted dooryard fences, extending from the house to the road. The

fence *e* was overthrown northward toward *f*, and the fence *f* in the contrary direction towards *e*; both directions being *transverse* to the line of the axis, which passes between them. Such cases have been adduced as supporting a directly inward course of the wind in the body of the tornado; or as indicating two bodies of opposing wind meeting on a central line; but I draw a different conclusion.

Fig. 1. *Providence Tornado**.



In this figure *a* represents a wooden dwelling-house of two stories with chimney at its centre; *b* a dwelling added to *a* and extending to the rear; *c* a lighter building about 16 feet by 30, attached to the rear of *b*: *g* was a large wooden barn: *h* a long building or shed extending from the barn to the carriage-house *i*. The width of the visible track was here about five hundred feet, and the course of the centre or axis of the tornado appeared to have passed somewhat diagonally over the three first-named buildings.

The house *a* withstood the shock, receiving some damage; the chimney top of *b* was thrown on the roof of *a*, perforating the same, while *b* was unroofed and greatly injured, and a long timber or *sill* from the shed *h* broke endwise into the upper part of the house *b* from a north-westerly direction. The building *c* was turned more than twenty feet to the *left about*, as regards the axis of the whirlwind, against the top of the prostrated pear-tree *d*, and was there overturned upon it. There were twenty-one persons in *a* and *b*, including a school of children, none of whom were seriously injured.

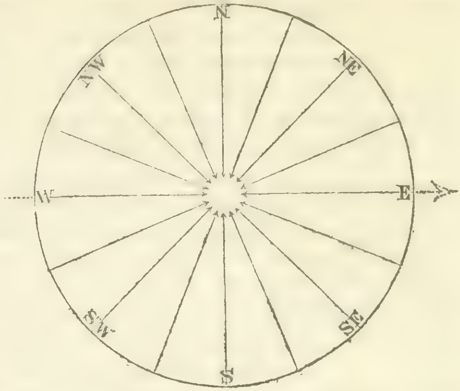
The barn *g* and the shed *h* were destroyed, and the materials swept off toward the first-named buildings. A corn-house, standing on the same side with the barn, is stated in the Providence papers to have been blown over to the *west*, but I can find no notes of my own respecting the direction of its fall.

Let fig. 2 represent, horizontally, the directions of such centre blowing winds in the body of the tornado, and let it be supposed as passing over the area of fig. 1, without revolving, so as the course of the centre will coincide with the arrow which indicates the course of the axis on that figure. It may thus be seen that on this hypothesis the wind must strike the fences *e*, *f*, either parallel to their length, or but little ob-

* On these plans the large dot at the end of the several short lines shows the original position of the root of the tree; the pointed end of the line shows the direction of its top.

lique; a direction of wind which seldom or never prostrates fences, even in the path of a tornado. Besides, *near the centre* of such an inward blowing tornado, where only it could act on these fences with *lateral* force, such winds must necessarily become neutralized both by blowing against each other and by turning upward to escape, thus having little effect at this point, within four feet of the ground. I say nothing here of the possibility of any winds blowing *with violence* in such central and opposing directions; which I could never conceive: for the entire spaces between the centripetal lines of arrows must be conceived as being filled by the affluent winds, the lines only indicating their directions.

Fig. 2.



But on the other hand, let us suppose a strong whirlwind passing in the same direction; the front half of which, both on and near the line pursued by its axis, must necessarily sweep laterally across this line, first *northwardly* towards *f*, if it be revolving to the left; and the last half of the whirl on its arrival will sweep *southwardly* towards *e*; which sufficiently accounts for the effects observed. That only the fence *e* was thus prostrated by the first wind of the tornado may be explained by the protection afforded to *f* by the house, against the *advancing* whirl, and perhaps here, also, by the spirally *upward* tendency towards the centre, in the wind which thus came round the south-east corner of the house, prostrating *e* in its course. But on the passing of the axis of the whirl, the wind would recur with increased force from the opposite direction, upon the fence *f*, prostrating it towards *e*; while the latter, being already down, and in turn partially protected by the house, would remain as it first fell.

In passing over the track of the tornado between Burr's house and Providence river, several instances and groups of prostration were observed; but owing to the open character of the grounds throughout most of the track, the memorials afforded by the trees were less frequent than have been seen in other cases.

Near the Pawtuxet turnpike the tornado encountered a new

house belonging to Mr. Gardner. This house was in the southern portion of the track *on the right of the axis*, and was removed and *turned* several feet *towards the left.*

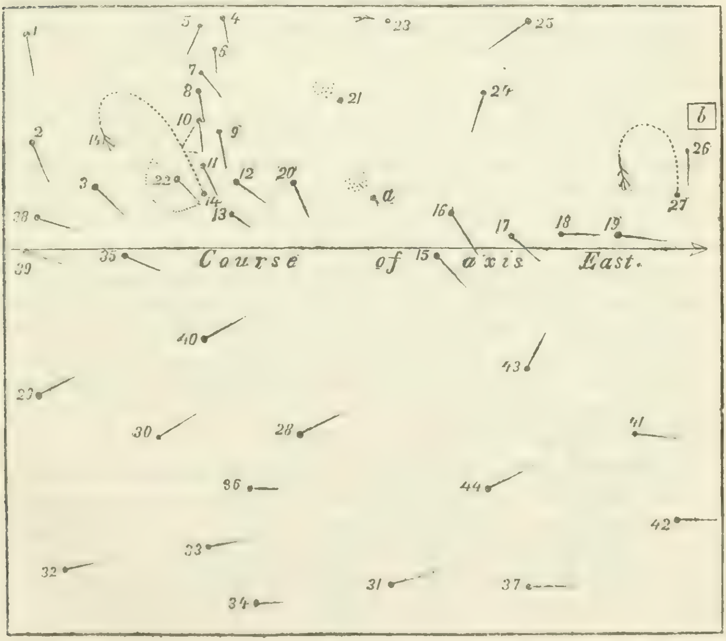
It is proper to mention here that the *order of changes* in the wind's *direction*, viewing the tornado either as a whirlwind, or as claimed by Mr. Espy and seen in figure 2, would at any *fixed point* on this the right side of the track, be successively *towards the right*, as relates to the centre of the tornado. But this building having received its motion by yielding to the wind, shows the true course of the latter as whirling to the left.

Passing by the prostration of the range of buildings near the river, described by Mr. Allen, I proceed to notice the effects which appeared on crossing to the Massachusetts side.

From the bank of the river to the house of Abraham Tifts on the Lyon farm, three-fourths of a mile, the grounds were open and unbroken, being mostly under cultivation and with few trees exposed to the tornado, excepting an orchard of scattered apple-trees westward of Tifts' house. The traces of the wind in and adjacent to this orchard were very distinct in their character, and I subjoin here the sketch on which they are represented.

Fig. 3. *Providence Tornado.*

North or left side.



South or right side.

Explanations of Fig. 3.—The cases of prostration 4 to 14, were from a line of small locust-trees on the west border of an old apple orchard, and are severally shifted a little out of line for the sake of a distinct exhibition of their directions.

From thence to near Tifts' house at *b*, the ground is but slightly foreshortened, and the relative positions of each tree, on the left of the centre, are approximately shown. The figure was drawn from my field notes on account of the distinct phænomena which were exhibited on this part of the track, and which, in cases *a*, 14, 22, 21, 23, and 27, show conclusively the first action of the whirl *across the path of the axis*, and sweeping towards the northern border of the track. On the opposite or right side of the axis, southward of 15, there were no trees exposed, and the effects of the tornado were here visible only on the crops and fences. Therefore the cases shown on the figures south of the axis, and also westward of 22 on the left side, were brought in from the more western parts of the track between the orchard and the river, and include all the prostrations from the latter to Tifts' house; and their relative distances from the axis or centre of the track are but approximated.

Case 14 represents a small locust-tree broken off at an old wound near the root and carried *outward* and *backward* into the adjoining fallow field, having struck into the ground seven times in its course, leaving distinct traces. It was finally left at a point N. 57° W. from its stump, at the distance of forty yards, with its top turned southwardly, in conformity with its two last traces in the soft ground.

Case 10, a small locust-tree was prostrated S. 25° W., leaving its mark in the fallow ground. It was subsequently shifted, by the progressive change in the whirlwind, to S. 11° E.

Case *a*, an old apple-tree with but a single branch projecting southwardly from its trunk; this branch was taken off by the onset of the tornado and struck into the ground north-west from the trunk, depositing its apples at this spot. The limb itself was missing.—Case 21, apples deposited as in case *a*.

Case 22, a small wild cherry-tree was found lying on and against the stump of 14, having first been thrown *from the latter* by the onset of the wind and subsequently swung round by the south to its present position, as appeared by the impressions made in the ground. Its final position was such, as if occurring at the outset would have prevented 14 from being carried off north-westerly.—Case 23, the branch of an apple-tree was thrown west.—At *b* is shown the relative position of Tifts' house.

Case 27 shows the original position of a large pear-tree, the stem of which was broken off and first thrown northward, where it ploughed up the soft ground of the garden by its force, and continued its circuit to a point north-west of its original position, where it remained with its top turned toward the south.

For the purposes of a general comparison, the observed or first-known directions of the prostrations on the two sides of the track may be summed up as follows.

Left or North side of the Track.			*Right or South side of the Track.*		
Case.	Direction of first prostration.	Inclination inward and backward from course of tornado.	Case.	Direction of first prostration.	Inclination inward and backward from course of tornado.
38	S. 74°E.	16°	29	N. 65°E.	25°
39	S. 70 E.	20	32	N. 77 E.	13
35	S. 67 E.	23	30	N. 60 E.	30
1	S. 10 E.	80	33	N. 80 E.	10

Left or North side of the Track.

Case.	Direction of first prostration.	Inclination inward and backward from course of tornado.
2	S. 23° E.	67°
3	S. 45 E.	45
4	S. 12 E.	78
5	S. 35 W. (backward)	125
6	S. 5 E.	85
7	S. 40 E.	50
8	S. 11 E.	79
9	S. 10 E.	80
10	fell S. 25 W. turned to S. 11 E.	115
11	S. 26° E.	64
12	S. 55 E.	35
13	S. 55 E.	35
14	first thrown N. 23 W. (backward)	247
15	S. 45° E.	45
16	S. 30 E.	60
17	S. 55 E.	35
18	East.	0
19	S. 85 E.	5
20	S. 27 E.	63
21	N. 55 W. (backward)	215
22	first fell N. W. turned to S. 37 E.	225
a	N. 45° W. (backward)	225
23	Branch of apple-tree thrown west	183
24	S. 20 W. (backward)	110
25	S. 55 W. (backward)	145
26	South.	90
27	first thrown N. 10 W.	260

Right or South side of the Track.

Case.	Direction of first prostration.	Inclination inward and backward from course of tornado.
34	N. 88° E.	2°
36	East.	0
40	N. 65 E.	25
28	N. 63 E.	27
31	N. 75 E.	15
44	N. 63 E.	27
37	N. 87 E.	13
43	N. 30 E.	60
41	S. 85 E.	—5
42	East.	0

Mean direction of prostration on the right side of the track, N. 73° E.; average inclination inward from course of tornado, *seventeen degrees*.

Mean direction of first prostrations on the *left side* of track, S. 4° W.; average inclination inward and backward from course of tornado, *ninety-four degrees*.

Relative inclinations of the two sides to the line of axis, more than *five* to *one*.

It is proper to mention, that the average inward inclination of *all* the prostrations on the *right* side of the track for a distance of four miles east of the river was thirty degrees*. This however does not affect the conclusions in favour of rotation to the left.

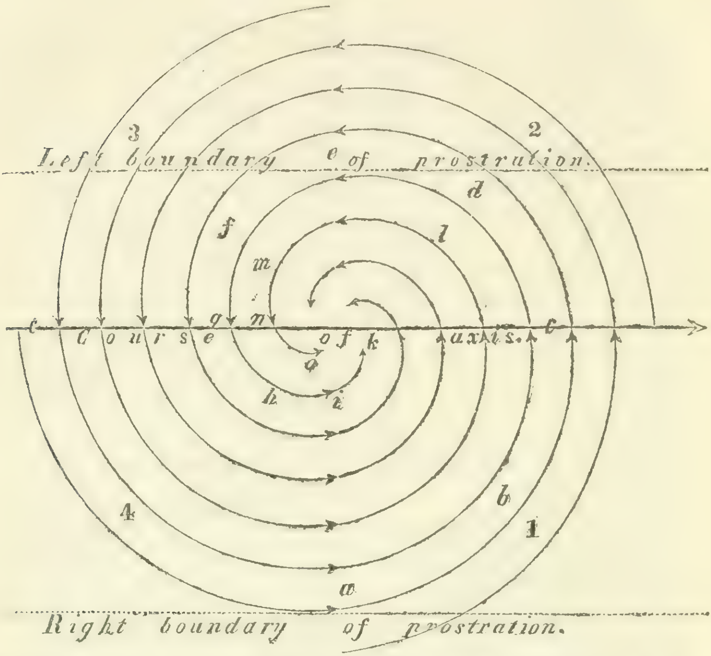
These average results, on the two sides, together with the several particular observations already adduced, appear to me to afford decisive evidence of whirlwind rotation in this tornado, in the direction from right to left, or which is contrary to the hands of a watch. In reference to this evidence and that exhibited in my paper on the New Brunswick tornado, I add from my prepared sketches the following figure, as an approximate illustration of the whirling action in these tornadoes, so far as this may be shown *horizontally* and by a stationary figure.

Let the involuted lines or arrows on this figure be supposed to represent the motion of the wind at or near the bottom of a vertically cylindrical portion of the centre of a tornado, com-

* This larger average gives a relative degree of inclination on the two sides of *three* to *one*. Nearly the same difference is found in two outside bands of prostration, of equal widths (Tables I. and V.), shown in my survey of the New Brunswick Tornado. See Silliman's Journal, vol. xli. p. 78.

prising a length of radius equal to the greatest width of the prostrating power on the right of the axis of its path. Now

Fig. 4.



if the tornado be considered as whirling in the manner here represented, but *without any change of location*, its action may be supposed as concentrically equal on all sides; the motion, however, becoming quickened towards the centre in the *inverse ratio* of the successive concentric areas; that is, each particle of air as it revolves about the axis, *continuing to describe nearly equal areas in equal times*, in its progress towards the centre, where it rises spirally in the direction of discharge; this direction being towards the point or area of least atmospheric resistance or pressure. Thus the course of a single particle, horizontally, may be *a b c d e f g h i k*;—and so on or between each of the four involuted lines which constitute the figure.

For further reference, we may divide this figure by the cross lines of arrow heads, into the four quadrants 1, 2, 3, 4.

We will now consider this whirl as having *a constant progressive motion* on the line of the long arrow *c c*, at a rate equal to one fourth or fifth of its average rotative velocity. It will then follow, that as the force of the whirl on the trees and

other objects encountered by it, is *as the square* of the wind's velocity at the point of impingement, the relative effects on the two sides of the line of the axis, which before were equal, will now be greatly altered.

For, if at a given distance *on the right* of the advancing axis, the former velocity was 80, it will now, as relates to the earth's surface, have become 100; and at the same distance on the *left* side the velocity of the wind will be reduced to 60, as relates to the earth's surface. Thus the squares of these effective velocities will give a power relatively equal to 100 at the former point and only 36 at the latter; both being equally distant from the axis. Hence, although the *rotative* velocity of the whirl decreases rapidly as we recede from its axis, yet its *prostrating* power will, by its progressive motion, become greatly extended on the *right* side of the advancing axis, and proportionally contracted on the left side. Thus the respective boundaries of the prostrating power on the two sides of the tornado, when thus in motion, may be those indicated on the figure; which nearly correspond to the effects which have been observed in several cases.

It may be seen further, that *nearly all the prostrations* near the line of the axis and elsewhere, must, by the advancing motion of the tornado, receive a direction *more onward* than is represented by the arrows or lines in the figure, which can represent only a stationary rotation.

In further considering these effects, in different portions of the whirl, as it encounters objects in its advance, we shall find the maximum effects to be mainly on the line *a, i, o*, at the rear of the *first quadrant*. Hence, if a tree on this side the axis should fail to be prostrated till after the first quadrant had passed over, it would not be likely to fall in the fourth quadrant, on the further advance of the tornado, unless very near to its axis. Moreover, if one tree should fall when under the more advanced portion of the first quadrant, another, if prostrated later in the same quadrant, must necessarily fall in a *more onward* direction than the first, and if sufficiently near will lie across the latter.

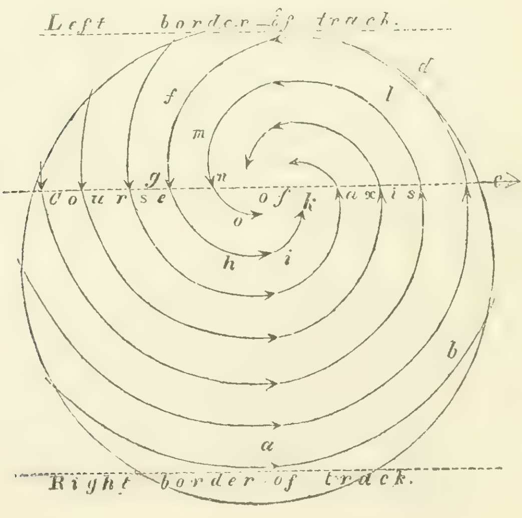
It may likewise be seen, that the wind of the whirl, in passing into the *second quadrant*, on the left side of the track, is sweeping *backward*, and with its effective power thus greatly reduced, as regards fixed objects on the earth's surface. Thus the limits of prostration are not only narrowed, but the effective power is here greatly reduced, and gives *fewer prostrations* than under either the first or third quadrants. The *minimum* of effect occurs on the arrival of the line *e k*, at the rear of the second quadrant.

But on the arrival of the *third quadrant*, the prostrating power on the left side becomes more and more efficient by the ceasing of the backward and the accession of the progressive movement; and at or near the line of *f m*, it again takes effect with rapid increase. The destructive force is also much augmented here by the greater velocity in the heart of the whirl, near its axis, and the impetus must rapidly increase in energy to its maximum effect, as at *m n o*, taking off any tree which may here remain, and carrying aloft, or sweeping onward, the objects previously prostrated on the line *c x k*.

If a tree on the *left* side of the track falls on one previously thrown down by the tornado, the last fallen will also have the more onward direction, as on the other side; unless both have fallen in the second quadrant, where few prostrations occur.—The *fourth quadrant*, for causes noticed in considering the *first*, can have little prostrating effect, except perhaps on the small area near its axis.

If we now conceive of our figure as applied only to the limits of prostration or destruction which constitute the *visible path* of the tornado, it becomes relatively unequal in its right and left-hand quadrants, the axis appearing greatly eccentric, and in the same degree, at least, as the *left* band or belt of prostrations is found narrower than that on the right of the axis. This apparent, but illusive form of the whirl, may be illustrated by fig. 5; which is drawn on the same lines with the preceding figure.

Fig. 5.



It will readily be seen that this eccentricity of the axis, on the visible track, will be in proportion to *the progressive velocity of the tornado*; other things being equal. Thus, if Mr. Allen be nearly right in his estimate of the rate of progress in the Providence tornado, the eccentricity of the axis of its path will be generally less than is shown in figures 4 and 5. On the other hand, if the progressive velocity should be as great as Professor Loomis informs me he ascribes to the tornado of February last, in Ohio, viz. about forty miles an hour, the eccentricity would in such a case be greatly increased, showing the axis as far outward, perhaps, as would be in line with m, or $f\,l$, in these two figures.

From this examination it appears to result, that an observer who follows the track of a tornado after its departure, will find on one side of the apparent axis of its path, *if it be a whirlwind*, a continued series of prostrations pointing almost invariably onward and inward, with various degrees of inclination to the course of the path; while on the other side of the axis a narrower band or belt of prostrations will be found, which are also inclined mainly inward and onward, but showing greater inclinations from the line of progress, together with frequent cases which incline more or less backward and sometimes even outward from the course of the tornado.

It may also appear, that a want of proper attention to the necessary conditions of the prostrating power in a progressive whirlwind, can alone induce us to ascribe such effects to supposed antagonistic winds, blowing simultaneously in opposing directions.

Leaving, for a moment, the more tangible features of this inquiry, we may now take some notice of the more outward portions of the "cone" or whirlwind, which are supposed not to be comprised in figure 4. Assuming here the involuted and inward motion, with its upward discharge at the centre, it follows that the impulsive accession of air which is necessary for maintaining a violent whirlwind action, must come in horizontally, and in the same gradually involuted courses; or must descend in like manner from a higher region, in and around the outward parts of the whirling cone. I have long since been led to believe that this impulsive accession comes from *both* these sources, but *chiefly from the latter*; and that this motion of accession and support is spirally *downward* in the outward portions of the whirl; the latter being, in its higher portions, often greatly expanded, as noticed by Mr. Allen.

The evidence on which this opinion rests, can be but partially alluded to here; but I will suggest the following con-

siderations:—1. The ascertained existence of a stratum of unusually cold air in the higher region of clouds, on some particular days remarkable for the occurrence of numerous thunder gusts and tornadoes*. 2. The observed descent of a portion of the clouds in front of the nucleus or body of a heavy squall or tornado, which may sometimes be traced by the eye as low as the existing limit of condensation will afford opportunity for observation. 3. The fact noticed by Mr. Allen and others, that adjacent "to the exterior edge of the circle of the tornado" or whirlwind, the previous breeze often continues "to blow uninterruptedly from the same quarter" as before†. 4. The last fact, when taken also in connexion with certain peculiar and striking effects in the outward portions or edge of the tornado, a knowledge of which I have gathered from various sources. 5. The coldness of the air which has been noticed at the edge of a whirlwind. 6. The instant penetration of the lower end of the whirlwind into thick forests, and into hollows and ravines, which has been frequently noticed. 7. The direct memorials of downward action in the outward portions of the whirl which I have myself met with on the tracks of different tornadoes.

In most of the foregoing remarks it has been my design to view the tornado as it moves onward, in full action. Of the origin or incipient causes of the whirl, it is not necessary here to inquire; although some clue to these is perhaps afforded us in the considerations above noticed.

Recurring once more to the track of the Providence tornado, I have to state that eastward of Tifts' house the course of the track soon became S. 65° E. magnetic, for more than two miles. It then took the course of S. 75° E., and further onward the tornado passed directly over the house of Solomon Peck, about four miles from Providence. This house was partly unroofed; chimney thrown down; windows broken *inward*, as in many other cases; and much other damage was also done to Mr. Peck's property. In passing onward towards

* This change of upper temperature I think can be clearly made out on the day of the New Brunswick tornado, which was but one of many tornadoes and thunder gusts which appeared in this part of the United States on the same day; and on the preceding day in Illinois and other western states.

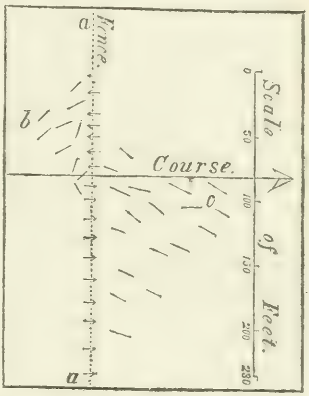
In the New Haven Gazette are accounts of *five* severe tornadoes which occurred in the states of New Jersey, Connecticut, Massachusetts and Rhode Island, on the afternoon of August 15, 1787. I can also refer to many more recent cases of this kind.

† The observation here quoted is one of many which show the error of the very hasty generalization which alleges a circuit or annulus of calm air to have been observed *on all sides* of tornadoes and hurricanes.

Taunton river the tornado appears to have preserved an inclination to the south of east; the track, though slightly sinuous, appearing, like that of the New Brunswick tornado, to form part of a great curve, with its convex side to the northward.

On the track from the Lyon farm to Peck's house there were many interesting memorials which might confirm the deductions already made. On some portions of the track, also, the tornado appeared to have risen almost entirely from the surface, its reversed apex leaving but a narrow trace, and on some fields even no trace at all. But in these cases, as on the tracks of other tornadoes, the compass bearing did not fail to lead the explorer to new ravages, where, at times, the energy of the tornado appeared to be greater than before*.

Fig. 6. *Providence Tornado.*



Before we take leave of the traces of this tornado I would adduce another of my prepared sketches, which shows the rotative effects in a manner which I think should satisfy the most strenuous opposer of whirlwind action.

In this sketch, fig. 6, we have represented a portion of the track which crossed at right angles a line of weak post-and-rail fence, *a*, *a*. On the *right* of the axis, this fence was prostrated *eastwardly* or in the direction of the course of the tornado, as shown by the short arrows which may represent the posts of the fence; the rails also having been scattered onward and inward, towards *c*, in the general manner represented in the figure. On the *left* side, however, every post was prostrated *westwardly*, and the rails were likewise blown slightly backward toward *b*, in the same general direction. The scale of feet, which measures across the track, was obtained by estimating twelve feet to each length of the rails. The locality of this sketch was perhaps a mile eastward of the Lyon farm.

The application of the foregoing views of rotation to this case, it can hardly be necessary to point out.

I have noticed many effects of similar kind on fences; but that the *backward* prostration on the left side of the track

* This is not uncommon in tornadoes, and is especially noticed in the account of two "Trombes" which are given in Pouillet, *Elémens de Physique et de Météorologie*, § 655.

should have taken full effect in this case, and mainly, perhaps, under the second quadrant, I ascribe to the age and general weakness of the fence.

Additional memorials might here be adduced in evidence, and of similar character to the foregoing; but having already occupied more space than I intended, I must now leave the question of a general whirlwind rotation in this and other tornadoes to the candid consideration of impartial inquirers.

New York, July 12, 1842.

IX. *Notices of the Results of the Labours of Continental Chemists. By* Messrs. W. FRANCIS *and* T. G. TILLEY*.

[Continued from vol. xxi. p. 452.]

On Perhydrosulphocyanic Acid.

M. VÖLCKEL has made some investigations of this body. He recommends the following as the best means of its preparation:—A cold saturated solution of the sulphocyanide of potassium in water is mixed with from 6 to 8 times its volume of strong hydrochloric acid, and allowed to stand for 24 hours. This mixture becomes thick, and after some minutes assumes a yellow colour. In about an hour it loses its glutinosity, giving out carbonic and hydrocyanic acids, and becoming a mass of small acicular crystals. To obtain the perhydrosulphocyanic acid pure, these are to be washed with cold water.

It is slightly soluble in cold water, but perfectly so in hot, out of which it crystallizes on cooling in yellow needles. It is also soluble in æther and alcohol. These solutions react slightly acid, and give reactions with metallic salts; with acetate of lead, yellow; the same with nitrate of silver, in the latter changing easily in colour, owing to the formation of the sulphuret of that metal; with perchloride of mercury, yellowish white; with sulphate of copper, yellow; with chloride of tin, yellow; and with solution of platinum, brown-yellow. It does not give precipitates with the other metallic salts.

He found the composition to be $C^2\,N^2\,H^2\,S^3$, which results agree with those of Woskrensky. To ascertain the atomic weight the lead salt was examined: it was found to be $C^2\,N^2\,S^3\,Pb$. The salt was obtained by precipitating a solution of the acid in water, by acetate of lead. The atomic weight is 2226·72, the equivalent of hydrogen being replaced by 1 atom of lead.

* My friend Mr. Croft having been called from this country by his appointment to the Professorship of Chemistry in the University of Toronto, these notices will be continued in future, by Mr. Tilley and myself.—W. FRANCIS.

Besides this neutral compound, this chemist has formed another, by precipitating a solution of the acid with basic acetate of lead. It is composed of 2 atoms of the neutral salt combined with 1 atom of oxide of lead.

$$C^4\ N^4\ S^6\ Pb^3\ O = 2\ (C^2\ N^2\ S^2 + Pb\,S) + Pb\,O.$$

If hydrochloric acid gas is passed through a saturated solution of the sulphocyanide of potassium artificially cooled, large quantities of the perhydrosulphocyanic acid are formed, with hydrocyanic acid, formic acid, carbonic acid, sulphuret of carbon and ammonia; but no sulphuretted hydrogen. Sulphuret of carbon and carbonic acid sometimes are produced in very small quantities, sometimes not at all.

3 atoms of hydrosulphocyanic acid are decomposed by hydrochloric acid into 2 atoms of perhydrosulphocyanic acid and 1 atom of hydrocyanic acid. The hydrochloric acid removes the water necessary for the existence of the hydrosulphocyanic acid. The formation of carbonic acid and the sulphuret of carbon depends on another decomposition. 1 atom of hydrosulphocyanic acid takes 2 atoms of water, and forms 1 atom carbonic acid, 1 atom sulphuret of carbon, and 1 atom of ammonia. If the solution of sulphocyanide of potassium be not sufficiently cool nor saturated, this decomposition takes place. The best proportions are 1 part of the salt and 5 parts water.

When heated to 150° perhydrosulphocyanic acid begins to be decomposed, forming sulphuretted hydrogen and hydrosulphocyanic acid. When the temperature is raised to 200°, sulphur, and sulphuret of carbon are given off; if still greater heat is applied, ammonia is formed and mellon is left.

Decomposition of Sulphocyanide of Potassium by Chlorine in the presence of Water.

The experiments of Parnell* and Bengiesser have been repeated by Völckel. When a stream of chlorine is passed through a concentrated solution of sulphocyanide of potassium, the latter being kept cool, there are formed sulphuric acid, hydrocyanic acid, or cyanogen, and a yellowish red body, but no carbonic acid nor ammonia.

This body is insoluble in water, alcohol, and æther. It is dissolved with difficulty by diluted potash ley; when the mixture is heated the solution is easily effected, and is of a deep red colour. From this solution acids reprecipitate it unaltered. The analysis made by Völckel of this body agrees

* The experiments here referred to will be found in Mr. Parnell's paper on sulphocyanogen, Phil. Mag. S. 3. vol. xvii. p. 249.

with that of Parnell, but the formulæ which he draws are different. His numbers are as Parnell's,

$$C^8\ N^8\ H^4\ O^1\ S^8.$$

Placed under the microscope, it is, however, found to consist of two distinct substances,—a darker and a lighter one. He objects to Parnell's theory of the formation of the yellow body. It was found to contain hydrogen, and can therefore only be the result of the decomposition of a body containing that element.

He explains the decomposition thus:—1 atom of sulphocyanide of potassium is by the chlorine and water decomposed into cyanogen, sulphuric acid, and hydrochloric acid. These acids decompose another part of the salt, and set free hydrosulphocyanic acid; by the action of chlorine on this, the yellow body is formed.

If sulphocyanic acid is acted upon in the heat by chlorine, another body is formed, possessing nearly all the properties of the yellow substance described above, except that it is slightly soluble in boiling alcohol. It has for its formula

$$C^8\ N^8\ H^6\ O^1\ S^9.$$

Decomposition of Sulphocyanuret of Potassium by Nitric Acid.

The action of nitric acid on this salt produces large quantities of carbonic acid and binoxide of nitrogen. The fluid contains sulphuric acid and ammonia, and a yellow body resembling the one last described: C, 18·85; H, 1·25; S, 54·11.

Völckel considers sulphocyanogen as a sulpho-acid of cyanogen, and considers its salts as sulpho-salts, of which the type is sulphocyanic acid, $C^2\ N^2\ S + H^2\ S$, the H^2 of which can be replaced by metals. The perhydrosulphocyanic acid is a hydrosulphuret of another sulpho-acid of cyanogen $= C^2\ N^2\ S^2 + H^2\ S$. (*Ann. der Ch. und Ph.* vol. xliii. S. 73–106.)

Dimorphism of Sulphur.

Marchand and Scheerer have made some experiments on the physical properties of sulphur in its different conditions.

Of sulphur obtained by crystallization from sulphuret of carbon, in five experiments they find the sp. gr. to be 2·049.

Of natural sulphur ... ... 2·066.

Of sulphur, which having been melted had been allowed to become again yellow, in six experiments ... ... 2·043.

They found the sp. gr. of the brown sulphur to be 1·99, but during the process of its change into the yellow the sp. gr. rose to 2·05.

They have ascertained the quantity of heat developed in this

change in density, and find it to be 1·35 per cent., which agrees well with the sp. gr. found.

The sp. gr. of the soft sulphur they found = 1·959. It rose as the sulphur became hard to 1·98, and when it assumed the yellow colour the density was 2·041.

They found the specific heat of the brown sulphur to be to that of the yellow as 1·021 : 1.

The solidifying point of sulphur was found to be 111·5 C.; and during the solidification the temperature rose to 113°.

Wœhler has in many cases of dimorphism shown that the temperature required for fusion varies in the different conditions of the bodies.*

Marchand and Bunsen found the point of fusion of brown sulphur to be 112° C., and of the yellow modification 113° C., (*Journal für Praktische Chemie*, xxiv. S. 129–156; *und* xxv. S. 395–398.)

X. "*On the Analytical Condition of Rectilinear Fluid Motion*," *in Reply to* Professor Challis. *By* G. G. Stokes, *B.A.*, *Fellow of Pembroke College, Cambridge*†.

PROFESSOR Challis has brought forward a new proof of his theorem, that when $u\,d\,x + v\,d\,y + w\,d\,z$ is an exact differential the surfaces of displacement are surfaces of equal velocity, in which he has not defined the quantity r which he employs, but only $d\,r$. In order that the equation $s = \int V\,d\,r$ should hold good for all points of the arbitrary curve PQ, we must evidently have $r = \int \cos\theta\,d\,s$, where s is the arc of the curve, and θ the angle between a tangent to the curve at any point and the direction of motion at that point. Consequently, we must make a distinction between r along the curve P Q, and r along the trajectory P R. Let the former be denoted by r', and the latter by r; then

$$\text{velocity at R} = \frac{d\,s}{d\,r}, \text{ velocity at Q} = \frac{d\,s}{d\,r'}.$$

The proof therefore falls to the ground; for, to assume that $r = r'$ would be to assume the theorem to be proved.

I cannot see *where* Professor Challis conceives it to be proved that $d\,x$, $d\,y$, $d\,z$ are independent of the time, in the equation $u\,d\,x + v\,d\,y + w\,d\,z = 0$, which is the differential equation to a surface of displacement, supposing the first side an exact differential. The instance which he brings forward only proves that they *may* be independent of the time, which is not denied; but a single instance where they are not, such

* See the present number, p. 76. † Communicated by the Author.

as I gave in my last latter, is sufficient to prove the theorem to be untrue.

If the velocity common to all parts of a fluid *must* be eliminated before any use is made of the equations of fluid motion, how is this velocity to be defined? By conceiving a velocity equal and opposite to that of any arbitrary particle impressed on the whole mass, we may get an infinite number of such velocities.

Professor Challis has brought it forward as an objection to the cases in which $u\,dx + v\,dy + w\,dz$ is an exact differential, that Poisson has mentioned a case where this is not true, although the motion is small. But the theorem is only true, for the case of small motions, when the initial velocities u_0, v_0, w_0 are such that $u_0\,dx + v_0\,dy + w_0\,dz$ is an exact differential neglecting squares of small quantities. For the equation $\frac{d^2u}{dt\,dy} = \frac{d^2v}{dt\,dx}$ gives, on integrating with respect to t, $\frac{du}{dy} - \frac{dv}{dx} = \frac{du_0}{dy} - \frac{dv_0}{dx}$, and the same applies to the other two corresponding equations. This exception includes the case mentioned by Poisson.

XI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xxi. p. 561.]

February 23, 1842.—A MEMOIR was read, entitled, "Report on the Missourium now exhibiting at the Egyptian Hall, with an inquiry into the claims of the Tetracaulodon to generic distinction," by Richard Owen, Esq., F.G.S., &c.

The author commences with some remarks on the manner in which the Missourium is set up, and after pointing out certain mistakes, as well as the readiness with which Mr. Koch, the proprietor, corrected an error respecting the first pair of ribs, he states that the necessary reform in the juxtaposition of other parts of the skeleton could be effected only at a great expense.

Mr. Owen then proceeds to consider the species of animal to which the skeleton is to be referred. It was, he says, a mammiferous animal, and while the anterior extremities disprove the existence of clavicles, they establish that the fossil belonged to the Ungulata. The enormous tusks of the upper jaw further show that it was a member of the proboscidean group of Pachyderms, and that the molar teeth prove it to be identical with the *Tetracaulodon* or *Mastodon giganteum.* With respect to the horizontal position of the tusks in the skeleton exhibited at the Egyptian Hall, Mr. Owen states, that it may have arisen from compression, the tusk of the Mas-

todon, like that of the Elephant, being inserted by a nearly straight cylindrical base in a socket of corresponding form, and can be rotated in any given direction when the natural attachments are destroyed by decomposition; and he alludes to the skeleton exhibited in London in 1805, in which the tusks were bent downwards.

Having, by a series of comparisons of the teeth and bones, which the author does not conceive it necessary to recount, arrived at the conclusion that the Missourium is either a Tetracaulodon or [a] Mastodon, he next considers the relations in which these supposed distinct genera stood to each other; premising that Mr. Koch's skeleton illustrates the osteology of the gigantic Mastodon far more completely than has been done by any other collection of North American fossils brought to Europe. The genus Tetracaulodon was founded by Dr. Godman on the lower jaw of a young Proboscidean having two tusks projecting from the symphysial extremities. Mr. W. Cooper of New York, however, suggested that the Tetracaulodon was nothing but the young of the gigantic Mastodon, and that the tusks were lost as the animal became adult. This opinion has been also advanced by others, but without being illustrated by any analogies; and it has been opposed by Dr. Isaac Hays, in an elaborate memoir on additional specimens, which he states present all the proofs necessary for refuting the opinion that Dr. Godman had committed the error of describing as a new animal the young of a known species; and he observes with respect to Mr. Titian R. Peale's suggestion that the lower tusks might be only a sexual distinction, "that it is impossible in the existing state of our knowledge, and with our present materials, to confirm or positively refute this suggestion." The most recent opinion on the subject, Mr. Owen states, is contained in the last edition of the 'Ossemens Fossiles,' in which M. Laurillard, after alluding to the opinion that the lower jaws with tusks may be immature Mastodons, proceeds to say, "others have been led to believe that the lower jaws of every age which have tusks belong to a different species of large Mastodon: some characters taken from the form of the jaw would seem to justify that opinion."—*Oss. Foss.* 8vo. vol. ii. p. 373, 1836.

Mr. Koch's collection of detached bones contains, Mr. Owen states, a number of lower jaws with the molars of *Mastodon giganteum*, which prove the important fact, that an animal of the same size and molar dentition as the Mastodon was characterized in the adult state by a single tusk projecting from the symphysial extremity of the right ramus, and that the two inferior tusks are manifested only by immature animals.

Mr. Owen then details the evidence by which he arrived at the conclusion that the Tetracaulodon of Dr. Godman is the immature state of both sexes of the *Mastodon giganteum*, that in the adult male only one of the lower tusks is preserved, and that in the adult female both are wanting.

A table is given in the memoir of the measurements of six lower jaws of full-grown animals; three which retained the right tusk or exhibited its socket, and three in which the tusk was wanting, and

the socket more or less obliterated; and Mr. Owen says that the dimensions prove the close similarity in size and proportions between the lower jaws of Mastodons with and without the tusks; and further that no individuals of the same species could resemble each other more closely in the conformation of the molar teeth. In both, the inner boundaries of the molar series are parallel, and the interspace is of the same breadth: the general form of the ascending ramus and the symphysis, the place and size of the great foramina for the dental nerves and vessels, are alike. The only differences consist in the Tetracaulodon * having larger condyles, and the outer side of the horizontal ramus being less convex and prominent; the coronoid process also is higher; and the broad canal, which is impressed upon the upper part of the symphysis, is nearly straight, not sloping down to the deflected part as in the Mastodon; but the breadth of the canal is the same in both, though the symphysial part of the jaw is larger and broader in the Tetracaulodon than Mastodon. These differences, Mr. Owen observes, may relate to the additional motions of the lower jaw, connected with the uses to which the incisor may have been put.

The incisor in full-grown Tetracaulodons or male Mastodons is a comparatively small, cylindrical and straight tusk, projecting forwards and a little downwards; its circumference is five inches; the length of the projecting part of the most entire of three specimens was five inches, but an unknown portion had been broken off; the socket was three inches in depth, uniformly one and a half inch in diameter, and slightly concave at its termination.

With regard to these incisor teeth and the importance attached to them as a generic distinction, Prof. Owen says, it must be remembered that in many species, both of Cetacea and Pachyderms, incisors as well as canines vary in relation to the age and sex of the same species of animal. In the male Dugong the upper incisors are protruded, scalpriform, and of unlimited growth, while in the female they are concealed, cuspidate, and solid to their base. In both sexes the lower jaw is provided at its deflected extremity with six incisors, which disappear in mature animals, only one or two remnants being occasionally discoverable in the cancellous sockets. In many of the Hog tribe, incisors are present in the young animal, but are lost in the full-grown. The most remarkable case, Mr. Owen says, of distinct conditions of incisors, teeth or tusks, relative to age and sex, is in the Narwhal. In this animal the young of both sexes have equally developed on each side of the upper jaw a single tusk, one of which grows rapidly in the male, constituting the well-known long, spirally twisted tusk, while the other remains stationary; but both continue rudimental in the female.

Were the Dugong and the Narwhal extinct, and to be judged of only by their fossil remains, the skulls of the two sexes of the herbivorous cetacean, viewed irrelatively, would doubtless, Mr. Owen observes, be referred to two distinct species, though the identity in

* The author retains the term Tetracaulodon in his description for the male Mastodon.

the molar teeth might impress the more cautious palæontologist with a strong suspicion of their generic identity; but the cranium of the male Narwhal, with its unsymmetrical distortion, increased by an enormous tusk, would, it can scarcely be doubted, be referred to a genus of Cetaceans quite distinct from that which the edentulous and more symmetrical skull of the female would be considered to represent.

In determining the real nature of differences in these extinct animal remains, Mr. Owen says it is necessary to inquire what other modifications are associated with those of the tusks;—are the more essential parts of the dental system, as the grinding teeth, alike or different in the jaws with tusks and without tusks? Do the jaws themselves and the other parts of the skeleton offer the modifications of form which usually attend distinction of species? Above all, are the same characters presumed to distinguish the genera, present in the young as in the adult skulls? are there, for example, young Mastodons as well as young Tetracaulodons?

The youngest of five full-grown Tetracaulodons or male Mastodons, examined by him, had two molars and half of a third developed in each ramus; the first or antepenultimate having three transverse ridges, each divided into two tubercles; the second also three bicusped ridges; and the third two ridges extricated, and two others within the alveolar cavity. In the next jaw in the order of development, the third ridge of the last molar was extricated; in the third specimen the antepenultimate grinder had been shed, and the last molar exhibited the same degree of development; in the fourth jaw the ultimate molar was fully extricated, exhibiting four bicuspidate ridges and a talon; and the fifth or oldest Tetracaulodon retained its penultimate but worn grinders, the two anterior ridges of the last molars being a little abraded, and the talon being developed into a pair of small tubercles.

A series of jaws of female Mastodons (Mastodon proper of Dr. Godman and Dr. Hays) presented the same order of development.

Having already shown that the molar teeth are identical in number and form in the Mastodon and Tetracaulodon, Mr. Owen proceeds to point out their correspondence in the mode and order of succession. The lower jaws of both present, moreover, those characters by which the *Mastodon giganteum* is distinguished from the genus Elephas, namely, by the higher coronoid, the less-rounded angle, the straight inferior margin, the parallel inner alveolar border, and the more produced symphysial extremity. They present, besides, equally the minor characteristic of the sharp process on the inner side of the neck of the condyle, and the ridge continued from the outer side of the neck. Both have an oblong depression on the outside of the coronoid process, but varying in depth in different Tetracaulodons. In both the posterior aperture of the dental canal commences in the same place; and the inner side of the angle of the jaw is concave, and bounded by an irregular margin, indicating the attachment of the fascia covering the internal pterygoid muscle, the irregularity being stronger in the lower jaws of older individuals. The relative position of the principal anterior outlet of the dental canal is the

same in Tetracaulodon as in Mastodon, varying in both in its relative position to the teeth as these alter their position in age.

When the striking modifications by which the lower jaw of the Elephant differs from that of the Mastodon are considered, it cannot be supposed, observes Mr. Owen, that no corresponding differences should be present in the lower jaws of the Mastodon and of another genus of Proboscideans characterized by a difference in the number of the teeth, and he says, he knows of no analogy in the whole mammalian series that would justify such a belief. Tetracaulodons are as numerous in Mr. Koch's collection as Mastodons, yet there are not found in it two forms of humeri, ulnæ, radii, femora or tibiæ, only the merest difference of variety being detectable; whilst the femora of the *Elephas primigenius* associated with them are at once recognizable by modifications which might be expected to accompany true generic differences in the rest of the organization. With the exception of a few bones of the *Elephas primigenius*, all the other remains of proboscidian Pachyderms in Mr. Koch's collection, Mr. Owen is of opinion, belong to the *Mastodon giganteum*; and the great skeleton he considers to be that of a male individual, on account of the size of the tusks and the strongly marked external characters of the principal bones of the extremities; but he points out that the lower jaw belonged to a female, and he states that the proprietor acknowledged that it was not discovered with the other portions of the skeleton. The true height of the animal, taken at the dorsal spines, Mr. Owen estimates at ten feet, and the length, from the intermaxillary bones to the end of the sacrum, at sixteen feet, or four more than that of the Asiatic Elephant in the Hunterian Museum.

The supposed spinal column of a man fourteen feet high, Mr. Owen refers to the Lophiodon: Mr. Koch's collection also includes some interesting remains of the *Mylodon Harlani*, also portions of large species of Bos, Cervus, &c.

With respect to the use of the lower incisor, Mr. Owen says, if indeed this diminutive inferior tusk were a generic character constantly associated in both sexes with the enormous upper tusks, no explanation could be given of so apparently useless an appendage; but if regarded as a sexual character, there are in the animal kingdom abundant examples of the functional importance of external distinctions in the male; and such he considers to be the explanation of the persistent single or prominent tusk in the male Mastodon. Further, with respect to the question why two tusks should be originally developed, especially in the female, in which neither is to be retained, Mr. Owen replies that there is an equal difficulty with respect to the two rudimental tusks in the female Narwhal, and of the single one in the male; to the abortive incisors in the symphysial part of the lower jaw of the Dugong; to the rudimental teeth in the lower jaw of the Fœtal Whale-bone Whale; and in the upper jaw of the Sperm Whale. In these, and many analogous instances, the author observes, a structure which is merely sketched out, and is functionless in one species, is perfected and performs important uses in an-

other closely allied. Thus the teeth which are shadowed forth in the lower jaw of the Fœtal Whale are fully developed in the Cachalot. The upper rudimentary maxillary teeth which remain hidden in the gum of the Sperm Whale are functionally developed in the Grampus; and in like manner in the gigantic Dinotherium, discovered by Dr. Kaup, is exhibited the full and functional development of the inferior rudimental tusks of the Mastodon.

The molar teeth of the Mastodons offer, Mr. Owen says, a beautiful transitional modification connecting the lamellated structure of the triturating molar with those having simply a transversely-ridged grinding surface. The interval between the molar teeth of the Elephant and those of the Tapir is too great to have allowed their fundamental resemblance to have been detected in the existing creation; but a study of the extinct Pachyderms brings to light, he says, a beautiful series of gradations leading through the elephantoid Mastodon of Ava and the gigantic Mastodon of the Missouri to the Dinotherium, which it may be remembered was the gigantic Tapir of Cuvier. Moreover, he adds, the indication of the singular armature of the lower jaw of the Dinothere might be most closely discernible in that species of Mastodon which makes the nearest approach to the Dinothere in the form of the grinding teeth.

The report from which the above extracts have been taken had been completed when Mr. Owen received a copy of the notice* of Dr. Hays's description of Mr. Koch's collection. After an attentive perusal of this document, in which the generic distinctness of the Tetracaulodon is maintained, Mr. Owen has been only more convinced of the truth of his own theory; he, however, in justice to Dr. Hays, gives the arguments of that esteemed naturalist. Dr. Hays considers the existence of a single tusk in the lower jaw to be only an accidental occurrence, referring, as examples of two tusks, to the specimen described by Dr. Godman, and to that belonging to the Museum of the University of Virginia. Respecting this statement, Mr. Owen observes, that the jaw described by Dr. Godman is that of an immature individual, retaining on the left side the first small molar, and therefore affords no proof of the persistence of the two inferior tusks in the adult animal, or evidence of the accidental nature of the absence of the left tusk in the mature jaw. With regard to the specimen in the cabinet of the University of Virginia, he says, that if this belong to a mature animal it would be an unique specimen, and might be paralleled with cases on record of two projecting tusks in the male Narwhal, and considered by all naturalists to be accidental. Mr. Owen further calls attention to the figure of the specimen in pl. 27. fig. 2. of the Transactions of the American Philosophical Society (vol. iv.), where only the right tusk is represented, the left being merely indicated by a dark spot of corresponding size, of the nature of which the text is silent.

Respecting the symphysial portion of the jaw exhibiting the alveoli of two tusks, both much smaller than the alveolus of the right

* Proceedings, American Phil. Soc. October 1841.

tusk in the presumed male Mastodon's jaws of corresponding size, and considered by Dr. Hays to constitute a distinct variety, if not a new species of Tetracaulodon, Mr. Owen considers it to be the jaw of a young female Mastodon in which the obliteration of the tusks had not been completed.

A lower jaw without tusks, considered by Dr. Hays to have been a young Mastodon, but with "the chin slightly broken, so that it is impossible to determine whether it had the foliated termination so conspicuous in the adult;" Mr. Owen remarks, that notwithstanding the prominent end of the symphysial part containing the chief portion of the tusk-socket is wanting, yet "two foramina are recognized at the anterior part of the chin," and these, he observes, must be either portions of the alveoli of the tusks, or the canals of the nerves and vessels for the tusks in these alveoli.

Thus, Mr. Owen says in conclusion, all the examples which seemed to show that the genus Mastodon at no period of life possessed tusks in the lower jaw, and that the genus Tetracaulodon was characterized at all periods of life by two projecting tusks in the lower jaw, become invalidated on a close inspection, and enter into the series of facts which support the proposition that the *Mastodon giganteum* has two lower tusks originally in both sexes, and retains the right lower tusk only in the adult male.

March 9.—The following communications were read:

1. A paper "On the Salt Steppe south of Orenburg, and on a remarkable Freezing Cavern." By Roderick Impey Murchison, Esq., Pres. G.S. [An abstract of this paper has been inserted in vol. xxi. p. 357.]

2. Extracts from a letter addressed by Sir J. Herschel, Bart., F.G.S., to Mr. Murchison, explanatory of the Phænomena of the Freezing Cave of Illetzkaya Zatchita. [See p. 359 of vol. xxi.]

3. "On some Phænomena observed on Glaciers, and on the internal temperature of large Masses of Ice or Snow, with some Remarks on the natural Ice-caves which occur below the limit of perpetual Snow." By Sir John Herschel, Bart., F.G.S., &c. [See p. 362 of vol. xxi.]

A paper "On Rock-Basins in the Bed of the Toombuddra, Southern India (lat. 15° to 16° N.)," by Lieut. Newbold of the Madras Army, was then read.

Rock-basins abound in the beds of many rivers in southern India, particularly where rapids and falls are of frequent occurrence; but in none are they more numerous and better exhibited in their various stages of formation than in the Toombuddra. In the bed of this river, near the island of Desanur and below the falls caused by the anicut or ancient stone embankment thrown across the channel for purposes of irrigation, is a great number of these cavities generally of a circular and oval form, and of various dimensions, equal, in one instance, to 12 feet in circumference and 4 feet in depth. Upwards of 130 basins were observed here and near the ruins of Bijanugger (lat. 15° 14′, long. 76° 37′). On many large bare slabs of rock are chains of these cavities connected by

shallow channels worn in the granite in the direction of the current of water; and the author mentions in particular one, consisting of forty basin-shaped cavities near the ruins of the pavilion of the sixty columns at Annagundi (also lat. 15° 14′, long. 76° 37′). Below the anicut of Sanapore, near Bijanugger, where the river bursts through a natural barrier of granite, the rocks both on the bed and at the sides are honey-combed; and still higher up the river, below the anicut of Wullanapore (lat. 15° 6′ N. long. 76° 22′ E.), the gneiss forming the bed of the river is very greatly eroded, as well as the basalt of a dyke. The different effects of water on rocks dependent on their relative position, the author says, is forcibly illustrated in this part of the river. Above the anicut the bed of the Toombuddra is slightly inclined, the stream flowing in one smooth and majestic sheet nearly 300 yards in breadth over rocks, the surface of which is almost unimpaired, and a Hindu inscription, on which the waters have glided upwards of three centuries, retains its characters almost as fresh as if cut only a year, while, in the rapids below the anicut, the strata are perfectly honey-combed.

The interior diameter of these basins is generally larger than that of the orifice, resembling a compressed globular vessel, and at the bottom there is a conical projection 2 or 3 inches in height, somewhat resembling that of a common black wine-bottle. The part where the water enters is usually the deepest, and in old cavities the margin, as well as that on the opposite side, is often worn back; the sides, bottom and lips of the orifice are however smooth. The funnel-shaped cavities, which are more rare than the basin-shaped, almost invariably occur where a loose block of rock has been worn quite through, or where water falls on or near the point at which two actual fissures intersect the rock at considerable angles. Many of the superficial erosions resemble the hoof of a horse, having a frog-like projection in the centre. The largest rock-basin which Mr. Newbold had seen was 300 feet deep and 750 in circumference. It was in gneiss, and immediately below the great falls of Gairsippa, in the western Ghauts, where a river 100 yards broad and 10 feet deep falls, during the monsoons, over a scarp upwards of 1000 feet high.

It is during the period when the waters of the river begin to diminish and an endless succession of small cascades or rapids is formed, that most of the cavities are worn in the higher portions of the rocks, and when these are left dry and the bulk of the river is still further reduced, that the cavities at a lower level are acted upon for a time. Some rock masses, after having had holes worn on one face, and been subsequently detached from their position and inverted, and again eroded on another face, present the singular appearance of having cavities on upper and under surfaces. In the formation and enlargement of the basins, Mr. Newbold is of opinion, that the erosion is the work of water, assisted only by the effects of the atmosphere upon the rock during the dry season; and he thinks that these two agents are fully adequate to make the cavities with [without?] the aid of a natural decomposition of the strata or the attrition of blocks and pebbles rolled along by the current, though

he admits their cooperation to a certain extent. The manner in which he considers the basins are formed is as follows. The water having worn a hole, however small, in the rock, flows into it in a circling eddy, and thereby enlarges the sides and bottom in a greater ratio than the orifice. This mode of operation, he says, may be demonstrated by throwing a fragment of cork into the current before it enters the cavity, and by then watching the gyrations of the cork till it escapes over the lip of the basin. During this experiment it will be seen, that the centre of the bottom is but little acted upon, and that the projections before noticed are consequently left. That these cones do not rise to the level of the orifice, Mr. Newbold says, is accounted for by the action of the water in the shallow cavities being more equally distributed over the whole superficies of the interior, and from the formation of the projections not commencing till the basins have been deepened.

The cavities are mostly free from sediment, but some contain pebbles and sand disposed in a horizontal bed at the bottom, undisturbed by the rotatory motion of the water. In all cases in which Mr. Newbold noticed earthy matter carried into the basins by the current, the weightier pebbles sunk immediately, and either remained stationary or were but slightly moved; and the heavier particles of sand also sunk after making one or two whirls round the interior of the basin, while the mud and other light materials passed with the upper current over the lip at the opposite side.

During the dry seasons, when the contents of the basins are gradually evaporated, the carbonic acid contained in the water, acts, Mr. Newbold says, upon the rock which frequently possesses a temperature of 120°, and softening the interior of the cavity, prepares it for additional erosive effects by the river during the next monsoon.

Besides the river-basins, the author alludes to similar but smaller cavities on the surface of rocks at considerable elevations above the drainage level of the country, and which result from the action of springs or rain-water overflowing from receptacles where it had collected; also to other hollows not referable to similar agency, on the summits of table-lands and isolated mountain-peaks, where no springs or collections of rain-water have been known to exist. Cavities of this description the author has observed on the summit of limestone mountains in Greece, Sicily, the south of Spain, the opposite coast of Barbary; on the table summit of the Gebel Ataka range, on the west coast of the Red Sea, and in the granite rocks of Mount Sinai; and he refers them to diluvial action. Lastly, he refers to a remarkable funnel-shaped cavity at Malta, described by the Hon. Mr. Frere*, and ascribed by that author to a rush of water pouring down the cavity, though there are now no signs whence such a body of water was derived†.

* Edinb. Phil. Journ., January 1837, p. 23.

[† On the subject of the origin and mode of formation of rock-basins, Mr. Brayley's paper in Phil. Mag. S. 2. vol. viii. p. 331, may be compared with the above.—EDIT.]

Three Notices by Mr. J. Phillips, and communicated by J. Taylor, Esq., Treas. G.S., were then read.

1. The first of these communications gives an account of the Cave of Cuernavaca or Cacaguamilpas, thirty-two leagues S.S.W. from the city of Mexico, or sixteen from the town of Cuernavaca. It is situated in a range of limestone hills, and is of vast extent. A descent of fifty feet conducts from the entrance to the floor of the cavern, which for some distance is tolerably level, though covered with the debris of the limestone to a considerable depth; but the progress of the visitor is afterwards greatly impeded by huge piles of rocks apparently fallen from above. Enormous and fantastic stalactites and stalagmites abound on every side. At a spot where the cavern separates into two great branches, the height was estimated by means of rockets to exceed 200 feet; and the depth of the left branch is stated to be at least half a mile; but the right branch had not been explored.

With reference to the statement of a writer on Mexico *, that he did not expect to see many caverns, if any, and that he had met with very little limestone, Mr. Phillips observes, that besides the great cavern of Cacaguamilpas, there are several in the district of El Doctor; and that limestone abounds in various parts of Mexico, occurring, besides the range of hills noticed above, at Atotomilco el Grande, north of the city of Mexico; at La Calera, on the road to Guanaxuato; also near Xeres in the state of Zacaticas, and at Bolaños in the state of Xalisco. Fossils are said by the author to be very rare in Mexico, but he obtained a species of Astræa in the limestone of El Doctor.

2. The second notice was on the remains of elephants, and on an ancient causeway near Mexico. The waters of the lake having permanently subsided to some distance from the Hacienda of Chapingo, the proprietor commenced a canal to restore the communication. Twelve feet below the surface an ancient causeway was discovered, and two or three feet lower the fossil elephant; and other similar remains are said to have been afterwards obtained. Humboldt, in his 'Essai Politique,' mentions the discovery of fossil bones of elephants in cutting the great drainage canal of Mexico; the only new fact therefore, the author states, which his communication contains, is the finding of the causeway,—an indication of difference of level in former times.

3. The third notice contained an account of six specimens of pumice obtained in sinking a well near Perote in Mexico. The road from Vera Cruz to the city of Mexico traverses an extensive tract of table-land, 7700 feet above the level of the sea, along the foot of the mountains which stretch out from the volcano of Orizaba and the Coffre of Perote. The water being exceedingly bad and brackish, a well was sunk to obtain better, at a new inn between Perote and Santa Gertrudes. The depth of well was sixty varas,

* Silliman's Journal, vol. xvi. p. 159.

the first ten being sunk in sand and the debris of pumice; and the remainder in pumice and scoriæ intermixed with obsidian. At the depth at which water was obtained the pumice assumed a more compact structure.

March 23, 1842.—A paper was first read, "On the Coal-fields of Pennsylvania and Nova Scotia," by William Edmond Logan, Esq., F.G.S.

The objects of this paper are to give, 1st, a few particulars connected with the extent and character of the carboniferous deposits of Pennsylvania, and to point out the extension to the coal-fields of America of some facts bearing on the origin of coal, advanced by the author in a previous memoir on South Wales; and, 2ndly, to detail the results of his observations in Nova Scotia.

1. *Pennsylvania.*—The whole of the Pennsylvanian coal-fields have been carefully examined, the author says, by the corps of State Geological Surveyors, under the able direction of Prof. Rogers, to whose admirable reports he bears testimony; but he laments their not being accompanied by a general map. In the construction of a small plan to accompany his memoir, and compiled from different sources, the author says, he is solely indebted for the contour of the bituminous district to Mr. Leslie and Mr. McKinnaly, attached to the State Survey; and that in the delineation of the complicated anthracitic regions he has taken advantage of a manuscript map which he obtained from Mr. Fisher, a coal-surveyor of Pottsville.

The Pennsylvanian carboniferous district is only a portion of that great coal region which extends into Maryland, Virginia and Ohio. The greatest breadth of the main coal-field of Pennsylvania is from the Alleghany mountains to within a dozen leagues of the southern shore of Lake Erie; and its length from Coudersport on the north to the southern angle of the State is about 200 miles. There are, however, also four or five important detached carboniferous regions on the Atlantic side of the Alleghanies, besides numerous small ones. The coal-measures consist of micaceous sandstones, arenaceous, argillaceous and carbonaceous shales, and valuable bands of limestone. In the bituminous district, under 800 feet of unproductive strata, are about 10 seams of coal, having an aggregate thickness of 50 feet, the whole resting upon a hard, coarse conglomerate, which is from 800 to 1200 feet thick at its south-eastern development, but is considerably thinner to the north-west. Beneath the conglomerate is a deposit of red shale which varies in thickness from 3000 to less than 100 feet, and disappears, it is believed, to the south-west. The next formation in descending order, with the exception of an interposed bed of fossiliferous limestone, consists of massive sandstones, conglomerates and shales, and it possesses a more uniform thickness than the two next superior deposits. All these formations are considered by Prof. Rogers to constitute a carboniferous system, though no profitable coal exists below the uppermost deposit; the remains of plants however occur throughout, and one or more seams of coal about a foot thick exist in the red shales. This

system rests upon a great development of sandstones and limestones, called by Prof. Rogers the Appalachian system, and divided by him into the following nine formations :—

1. Red and buff-coloured shales and argillaceous sandstones.
2. Olivaceous shales.
3. Fossiliferous sandstones.
4. Argillaceous limestone.
5. Variegated calcareous shales.
6. White and yellowish fucoidal sandstones.
7. Red argillaceous shales, with soft and hard sandstones.
8. Blue, drab and yellow shales.
9. Blue limestone.

The aggregate thickness of these deposits is stated to be upwards of 20,000 feet, and the whole of the formations from the top of the coal-measures downwards, to constitute one conformable series. The bottom limestone (No. 9.) has a wide range, extending through New York to the banks of the St. Lawrence, and it is believed, on account of its fossil contents, to belong to the lower Silurian series.

The entire 13 formations constitute a gigantic trough, the axis of which strikes from N.E. to S.W.; and along the N.E. outcrop of the carboniferous measures it has several deep indentations, occasioned, according to the observations of the State Surveyors, by a series of remarkable curvilinear, anticlinal axes, distant 10 or 12 miles from each other, and which preserve not only a parallelism among themselves, but, with the Alleghany and Appalachian mountains, increasing also in sharpness and importance as they approach these chains. The north-western anticlinal is the least conspicuous, but its effect on the margin of the coal-field is very perceptible; the 2nd has been traced 125 miles from the northern boundary of the State; the 3rd 160, the 4th 200, each penetrating to a greater extent within the coal area, and then flattening down; the 5th and 6th have been ascertained to have a range of 250 miles from the county of Susquehanna, and to traverse the whole of the coal district to the southern boundary of the State; but the 7th has been traced only 60 miles, or from the confines of Pennsylvania with Virginia to the Alleghany mountains, one of the ridges of which is considered to be a continuation of it. The different effect of these corrugations is stated to be remarkable. In the southern portion of the State, they have produced anticlinal hills and synclinal valleys; but in the northern, anticlinal valleys and synclinal hills; while mid-way there is a debateable land, which sometimes presents one set of phænomena, sometimes the other. These different conditions, the author says, are assignable to the nature of the formations acted upon; thus, where the anticlinal lines constitute hills they consist of the hard quartzose conglomerate underlying the coal strata; but where they occur in valleys, they are always connected with the soft portions of the coal-measures or the softer red shales.

The whole of the carboniferous regions above referred to, contain bituminous coal; but the detached districts on the Atlantic

side of the Alleghanies and eastward of the Susquehanna river produce anthracite, and it was to them that Mr. Logan more particularly directed his attention. These detached fields consist of a number of long, narrow, irregular troughs, separated by anticlinal axes of the quartzose conglomerate or subjacent red shale, and they are distinguished by the names of the southern, middle and northern anthracitic coal regions.

The southern region extends from Mauch Chunk on the Lehigh river nearly to Petersburgh on the Susquehanna, a distance of 70 miles, but its greatest breadth does not exceed six. It is traversed by five anticlinal axes parallel to one another and to that which bounds the trough, the steeper escarpment being on the north side; and the angle increases with each successive ridge, so that at the southern the strata have been elevated beyond the perpendicular and turned over, exposing beds many thousand feet below the coal-measures. Pottsville and Mount Carbon are mentioned as points where these phænomena are well exhibited. On inspecting the coal-seams in this neighbourhood, Mr. Logan observed, associated with every one he examined, similar stigmaria beds to those which he had previously described in his paper on South Wales; and he was enabled by them to detect the inverted position of the strata. The undulations in this coal-field render an estimate of the number of seams difficult, and Mr. Logan thinks that the 70 or 80 reported by the miners to exist, ought to be reduced to one-fifth. Some of the seams are of great dimensions, particularly that at the Room-Run and Summit mines near Mauch Chunk. The thickness of this deposit, with its associated partings of carbonaceous shale and an interposed stigmaria bed, is 50 feet, and it is estimated that the seam must yield from 40,000 to 50,000 tons per acre. At the Summit mines the coal is quarried to open day. Beneath the entire mass is a thick bed of underclay filled with Stigmariæ, and the occurrence of a similar bed 7 or 8 feet above the bottom of the coal, supports, Mr. Logan says, the opinion of Prof. Rogers and the miners, that the deposit in its progress westward splits into more than one workable seam.

The middle anthracitic coal region consists of an aggregate of narrow troughs, also separated by ten parallel anticlinal ridges or "geological wrinkles;" and the troughs are divisible into the western and eastern groups. The former, having an area of forty-five miles by five, comprises the Shamokin and Mahony coal-fields, as well as the basin of Sheenandoah valley, with several small districts; and the eastern group, with an area of twenty miles by five, consists of the coal-basins of Beaver Meadow, Duck Creek, Hazle Valley, Black Creek, Bucks Mountain, and McCauley's Mountain. From the frequency with which the conglomerate is brought to the surface, Mr. Logan says, it may be inferred that the middle region is shallower than the southern.

The northern anthracitic region, bounded like the others by the quartzose conglomerate, is crescent-shaped, and includes the beautiful valley of Wyoming. Its length, from Carbondale to Knob

Mountain, is fifty-five miles, but its greatest breadth is about four. Mr. Logan states that he took some pains to make a section across the northern region at Wilkesbarre, where the strata are less disturbed than in the two more southern regions. The total thickness of the measures, from the highest visible coal-seam to the quartzose, is stated to be about 2000 feet. The upper part, comprising about 650 feet, consists of argillaceous and arenaceous shales and sandstones, with only thin seams of workable coal, and it is well exposed near Wilkesbarre: the middle portion, also containing about 650 feet, is considered to be composed of softer materials, on account of its flatness; but its constituent strata are not exposed, and only one coal-seam four feet thick is reputed to occur in it. The lower measures consist of 700 feet of sandstones, with beds of shale, and they contain by far the most valuable coal-seams in the whole deposit, amounting to fourteen or fifteen in number, with an aggregate thickness of seventy to eighty feet. The most important bed, including interstratified shales and stigmaria beds, is thirty feet in vertical dimensions, but only eighteen are in general worked.

In concluding this portion of his paper, the author states, that he had seen nearly the whole of the anthracite seams mentioned in it, and that with only one exception had he failed to discover under the coal, wherever he could get to the bottom of the seam, a bed of argillo-arenaceous materials, generally fit for the purposes of fire-clay, and filled with *Stigmaria ficoides*. The character of the bed, he says, is known to the more intelligent miners of Pennsylvania, as it is to those of South Wales, and is called by them "bottom-slate." Professor Rogers, Mr. Logan adds, refers in one of his reports to a decided difference between "top-slate" and "bottom-slate," and, without alluding to the Stigmaria, remarks, that the "bottom-slate" is always composed of a strong tough material, having a peculiar splintery fracture, due, Mr. Logan says, to the vegetable remains; and he considers this observation of Professor Rogers an important collateral evidence in respect to the wide range of coal-deposits over which that geologist's examinations have extended. From information derived during personal communications with some of Prof. Rogers's geological assistants, Mr. Logan has no doubt that the stigmaria underclays prevail in the great bituminous districts of Pennsylvania; and he has been given to understand by Dr. Rogers, who conducts the Virginia survey, that similar beds of stigmaria clay constantly occur below the coal-seams in that State. In an appendix, sectional lists are given of various localities.

2. *Nova Scotia.*—Mr. Logan's examination of the coal-fields of this province was also made in the autumn of 1841, but subsequently to his visit to Pennsylvania. It was principally confined to the neighbourhood of Pictou (lat. 45° 48′, long. 62° 48′), which stands upon a carboniferous trough, and beneath which one seam, with a southwardly dip, is known to occur. At the Albion mines, ten miles to the south of the town, there is a great collection of coal-beds, which dip to the north. The number is stated by Judge Halibur-

ton* to be ten, and the aggregate thickness to be sixty feet. The only one at present worked contains twenty-four feet of clean coal, and about two hundred and forty tons of fuel are raised daily. Proceeding eastward the dip of the strata becomes more precipitous, and at New Glasgow, a distance of two miles, is a thick, highly inclined, very coarse quartzose conglomerate, considered by the miners to be a dyke, but by Mr. Logan to be a portion of the coal-measures. On Frazer's Mountain, to the east of New Glasgow, are two workable seams, measuring together about eight feet, and resting, with the interposition of a stigmaria bed, on a deposit consisting of non-fossiliferous limestone and sandstone. Judge Haliburton has given a detailed section of upwards of 600 feet of the strata at the Albion mines; and Mr. Logan, in an appendix, gives an elaborate list of beds, commencing 238 feet below Judge Haliburton's section, and extending in a descending series through upwards of 2500 feet. He is of opinion, that the whole series is susceptible of being divided into the following groups:—

1. Red and drab-coloured sandstones alternating with red and gray shales; a few coal-seams occurring chiefly towards the bottom, associated with limestone, and resting on a thick coarse conglomerate.	
2. Soft dark-coloured shales, with a few beds of sandstone, and richly stored with workable seams of coal and ironstone	5000 feet.
3. Marine limestone	10 ...
4. Coal-measures, probably unproductive, consisting in the upper part of red sandstones and shales, and of carbonaceous shales resting on stigmaria fire-clays; and in the lower, of red and gray sandstones, with a few bands of shale	1900 ...
5. Limestone	10 ...

All the above deposits contain carbonized vegetable remains, but in the beds next to be noticed they are rare.

6. Soft variegated shales, alternating in the lower part with red shales	650 feet.
7. Limestone	20 ...

Under every bed of coal which he examined, amounting to more than twelve, Mr. Logan detected the stigmaria fire-clay; and he was informed by Mr. Poole, the superintendent of the Albion mines, that similar strata occupy the same position in the coal-field of Cape Breton Island.

The limits of the Nova Scotia coal deposits have not been defined, and Mr. Logan states that considerable difficulties would attend an attempt to trace them, in consequence of the overlying gypsiferous strata. He believes that the Pictou field extends westward across Colchester county to the north side of the Basin of Mines, and that the seams which dip to the north at Kemptown and Onslow may belong to its southern side; he also believes that

* Statistical Account of Nova Scotia.

a parallel trough ranges to the southward, from Hawkesbury in Cape Breton Island to Windsor, on the south side of the Basin of Mines (lat. 44° 49′, long. 64° 19′ west); and three miles further south he also discovered coal-measures, rising with a northwardly dip of 45° from below the gypsiferous rocks, and resting on granite.

Coal is likewise reported to occur at Beaver Lake, south-east of the Albion mines, and to be brought down the East River during the spring floods, attached to floating ice. On the north side of the valley of the Stewiack, south-west of the Albion mines, coal-measures rest on a deep bed of limestone which dips to the south.

The gypsiferous strata, and the associated shales, sandstones, and fossiliferous limestones, Mr. Logan is of opinion, are not only newer than the coal-measures, but overlie them unconformably, founding his conclusions respecting the geological age of the formation on its organic contents. The fossils, he states, have been determined to be distinct from those of the carboniferous or any lower epoch, as well as from those of the lias or any superior deposit, but to have a decided generic agreement with the fossils of the triassic period.

At Horton Bluff, ten miles north of Windsor, and not far from gypsum beds, but between which and the point in question is a fault, some dark-coloured argillaceous strata alternating with calcareous bands are well exposed. Carbonized vegetable remains are not uncommon in these beds, which might easily be mistaken for coal-measures; but as one of the calcareous bands is nearly identical in character with the Windsor limestone, and as he also obtained a slab which appears to him to exhibit foot-marks, the author is inclined to consider the deposit as affording collateral evidence of the age of the gypsiferous strata.

A paper was afterwards read "On the Tchornoi Zem, or Black Earth of Central Russia," by R. I. Murchison, Esq., Pres. G.S. In this communication the author describes, first, the range and extent of the Black Earth; secondly, its chemical composition; and thirdly, he offers some remarks respecting its origin.

1. The northern boundary of the Tchornoi Zem may be defined by a line drawn in a curved direction from a little south of Lichwin (lat. 54° N., long. 33° 44′ E.) eastward to the Volga, in the 57th degree north latitude, occupying the left bank of that river west of Tcheboksar, between Nijni Novogorod and Kasan. It occurs also plentifully on the Kama and around Ufa; and on the Asiatic side of the Ural mountain it occupies an extensive district near Kamensk, in lat. 56°, and another between Miask and Sviask. Its northern and southern limits in Siberia were not ascertained, but it was found in the Baschir country on both flanks of the southern Ural, and in the steppes of the Kirghis. Between Orenburg and the mouth of the Volga it is wanting, the surface of the district consisting of marine detritus, containing shells of species which now exist in the Caspian: it appears to be equally wanting south of Tzaritzin on the Volga (lat. 48° 40′), as well as in the steppes of the Kalmucks, occurring in only very limited patches along the Sea of Azof, or south of the granitic steppes. It abounds, however, to

the north of that steppe, over a vast area, in broad valleys, on slopes and plateaux, and at all levels to the height of 400 feet; also on rocks of all ages, even overlapping the southern skirts of the great northern drift. It invariably constitutes the surface-soil, and is composed of black particles mixed with grains of sand, the former being so fine as to rise into the air even under the pressure of a horse's feet on the turf which covers it.

One of the marked features of all the alluvia of Russia characterises also the Tchornoi Zem. Wherever it occurs on plateaux or slopes, it is cut into by the ravines, called "avrachs" or "baltas," produced in the first instance by fissures formed during droughts, and widened as well as deepened subsequently by debacles arising from the melting of the thick falls of snow.

This black earth constitutes the finest soil in Russia, both for grass and wheat, and after yielding many crops in succession, it requires only a year or two of fallow to regain its fertility; the peasants, moreover, have prejudices against the use of manures, and Mr. Murchison believes that these feelings are strengthened by the natural productiveness of the soil.

2. *Chemical Composition.*—An analysis of a portion of the Tchornoi Zem, made by Mr. R. Phillips, Chemist to the Museum of Economic Geology, yielded in 100 parts, 69·8 silica, 13·5 alumina, 1·6 lime, 7· oxide of iron, 6·4 vegetable matter, and traces of humic acid, sulphuric and chlorine*. The black earth does not therefore differ, Mr. Murchison observes, in the composition of its solid contents from many of the red or brown soils of England.

3. *Origin of the Tchornoi Zem.*—In speculations on the origin of this deposit, the author dissents entirely from the opinion that it is due to decayed forests, as it never contains, even when exposed to the depth of 20 feet, any traces of trees, roots, or vegetable fibres, not connected with the existing vegetation. On the contrary, he believes it to be a subaqueous accumulation, but he objects to the views entertained by those geologists who place it, as respects its mode of production, on a parallel with the loess of the Rhine, or

* Since the paper was read the author has been favoured with an analysis by M. Payen, the celebrated French agricultural chemist. The following statement is a translation of part of M. Payen's communication:—

100 parts of earth.	6·95 combustible organic matter.				
	93·05 incombustible matter.	Soluble in boiling hydrochloric acid.	13·75	Alumina...	5·04
				Oxide of iron	5·62
				Lime	0·82
				Magnesia .	0·98
				Alkaline Chlorides	1·21
		Insoluble in boiling hydrochloric acid.	79·30	Silica......	71·56
				Alumina .	6·36
				Lime traces	—
				Magnesia .	0·24

The combustible organic matter indicated the presence, in 100 parts of the original earth, of water 4·81, azote 2·45, or together 7·26. The volume of the azote, M. Payen states, is remarkable.

with the upper diluvial mud of Belgium, France and Germany; not having anything in common with the latter, and differing from the loess by the absence of well-preserved freshwater and terrestrial shells indicating fluviatile or lacustrine origin. The loess, moreover, is never found on high plateaux; but Mr. Murchison does not dissent from the belief that the two deposits may have been produced at nearly the same epoch. He is therefore induced to consider the Tchornoi Zem as a submarine formation accumulated gently at the bottom of a sea undisturbed by any violent current, and beyond the range of those operations which spread out the northern drift. The absence of marine shells, he says, is only a negative objection, and not to be opposed to the evidence afforded by the widely different nature of the deposits at considerable levels, far above the drainage of the country or the action of any body of water which could occupy the valleys. Lastly, he ascribes the black colour of the earth to the state of decomposition of the vegetable matter originally diffused through the mud which now forms the fertile Tchornoi Zem of Russia.

XII. *Intelligence and Miscellaneous Articles.*

CURATORSHIP OF THE GEOLOGICAL SOCIETY OF LONDON.

We are happy in being enabled to state that the distinguished zoologist, Mr. E. Forbes, has been appointed Curator and Librarian of the Geological Society of London;—Mr. Forbes's name having been selected by the Council from a list of *nine* candidates, several of whom preferred very strong scientific claims. The choice of the Council was confirmed by an unanimous vote of a Special Meeting of the Society, held on the morning of the 14th of December.

We congratulate our geological friends on having secured the services of a naturalist who is qualified by his talents and acquirements to be a worthy successor of Mr. Lonsdale.

FORCE OF AQUEOUS VAPOUR.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Your Number for last month contains a paper by Dr. Apjohn, as read at the Royal Irish Academy, "On the force of aqueous vapour within the range of atmospheric temperature;" and in Table 1, which is for the correction of the mercurial column to 32° of Fahrenheit, I observe a wide difference in the result from what would have been the case from the method I have usually adopted, based upon the assertion of General Roy, for the expansion of mercury.

Perhaps Dr. Apjohn will have the kindness to explain, through your pages, his method of making the before-named calculations, and thereby confer a great obligation on,

Gentlemen, your obedient Servant,

Helston, Dec. 3, 1842. M. P. MOYLE.

ACETATE OF SODA CONTAINING NINE ATOMS OF WATER.

M. Anthon obtained during the summer some acetate of soda in fine acicular crystals, from a solution procured by the double decomposition of acetate of lead and sulphate of soda: this solution was rather dilute. He found, on examining the proportion of water of crystallization contained in this salt, that it amounted to 49·60 per cent., and which is much larger than that usually found to exist in it; the salt in question had been thoroughly dried by exposure to the air. In converting this proportion of water into atoms, it will appear that the following is the composition of this acetate:—

1 atom of soda	31·3	19·16
1 atom of acetic acid........	51·0	31·24
9 atoms of water	81·0	49·60
	163·3	100·

There consequently exists, according to this analysis, an acetate of soda which contains one and a half time more water of crystallization than the common acetate, which contains 6 atoms, or 39 per cent. of water.—*Repert. für die Pharm.*, and *Journal de Pharm.*, Novembre 1842.

ON THE SOLUBILITY OF ARSENIOUS ACID IN NITRIC ACID.

Many chemists have supposed that arsenious acid dissolves as readily in dilute nitric acid as in hydrochloric acid, without being converted into arsenic acid; M. H. Rose, however, states in his 'Analytic Chemistry,' that "nitric dissolves but a very minute quantity of arsenious acid without converting it, even when heated, into arsenic acid; this conversion is effected only by aqua regia." This assertion is true, provided the acid be not heated to ebullition, but requires correction with respect to a higher temperature.

M. Buchner triturated 4·6 grains of vitreous arsenious acid with about 46 grains of nitric acid of sp. gr. 1·230. The solution was not perfect, neither was nitric oxide given out. The mixture was afterwards heated in a matrass; nitric oxide was then evolved, red vapour formed, and the solution was readily and perfectly effected. It must therefore be admitted that one part of arsenious acid is totally soluble in 10 parts of boiling nitric acid of sp. gr. 1·230. It would, however, be incorrect to suppose that the arsenious acid is not oxygenated in this case; and the extrication of nitrous vapours proves the contrary. The author always found that on cooling a portion of the arsenious acid was precipitated from solution in a pulverulent state, and the solution still contained arsenious acid besides the arsenic acid formed. It is, however, unquestionably true that arsenious acid dissolves in much larger quantity and more readily in hydrochloric acid than in any oxiacid whatever, probably on account of the formation of chloride of arsenic; potash and soda are, however, the best solvents of this acid, whenever it is required to be quickly dissolved, or in large quantity in the state of arsenious acid. It is on this account that M. Valentine Rose has recommended the employment of potash in judicial inquiries, in order to dissolve the arsenious acid entirely and to separate it from animal matters.—*Ibid.*

ACTION OF SOLUTIONS OF CHLORIDES AND AIR ON MERCURY.

We have given in previous Numbers the results of M. Mialhe's experiments on the action of chlorides on some mercurial compounds, and he states that he had nearly concluded his experiments when it occurred to him to try whether mercury itself would not be acted upon by this class of substances.

Experiment, he states, confirmed his suspicions, for he found that the solutions of the alkaline chlorides put into contact with mercury and atmospheric air always produced bichloride of mercury, the quantity of which was greater in proportion to the concentration of the solution of the chloride, and the more perfect state of division of the metal, but no effect is produced unless oxygen, that of the air being sufficient, is present.

1*st Experiment.*—Mercury treated with the solution of alkaline chlorides (described in our last Number as the *assay liquor*), gave by stove heat 0·4 part of sublimate.

2*nd Experiment.*—The above repeated with the mercury finely divided by mucilage, yielded 0·7 part of sublimate.

The researches already detailed sufficiently prove, in the opinion of M. Mialhe, that the decomposing power of the alkaline chlorides is great, but they do not teach us anything as to their relative energy. The following experiments will supply this deficiency.

Hydrochlorate of Ammonia.—One hundred and twenty parts of hydrochlorate of ammonia and 30 parts of calomel were placed in an open bottle containing 1000 parts of distilled water, the temperature of which was gradually raised to 122° Fahr., and kept at this heat for half an hour; the sublimate produced amounted to 0·9 of a part.

The experiment repeated with the following salts gave the annexed quantities of sublimate:—

Chloride of Sodium ..	0·4	of a part.
Chloride of Barium ..	0·4	..
Chloride of Potassium	0·3	..

It results from these experiments that the hydrochlorate of ammonia is the most powerful of these four salts.

In concluding his experiments, M. Mialhe remarks that the reactions which he has pointed out take place at common temperatures, but better at the temperature of the human body. All of them are produced in a short time, and some occur instantaneously, the greater part requiring only a few hours' contact for action. As then the different fluids contained in the human body contain oxygen, chloride of sodium, and hydrochlorate of ammonia, accompanied or not with hydrochloric and other acids which may facilitate their action, it follows that all the chemical phænomena produced under the circumstances described, occur in the human body when any mercurial preparation whatever is introduced into it; these always produce a certain quantity of corrosive sublimate in which their medicinal properties reside; and this fact explains, in the opinion of M. Mialhe, the hitherto unexplained physiological action and therapeutic properties of metallic mercury when introduced into the animal œconomy. —*Ann. de Chim. et de Phys.*, Juin 1842.

DIFFERENCE BETWEEN THE FUSING POINTS OF THE SAME BODIES WHEN CRYSTALLIZED AND WHEN AMORPHOUS.

M. Wöhler having observed that lithofellic acid possessed different fusing-points when amorphous and when crystallized, extended his observations to other substances, and he has arrived at the conclusion that all dimorphous bodies have two different fusing-points. In passing from the crystallized to the amorphous state, bodies change all their physical properties, as colour, density, refractive power, specific gravity and solubility, but without any sensible alteration in the chemical properties. The following are the different temperatures at which the under-mentioned bodies fuse in their different states :—

	Crystallized.	Amorphous.
Sugar	320° Fahr.	194° to 212° Fahr.
Amygdalin	392 ...	257 .. 266 ...
Silvic acid......	284 ...	194 .. 230 ...
Lithofellic acid ..	401 ...	221 .. 230 ...

M. Wöhler observes that it is extremely difficult to determine with exactitude the fusing point of amorphous bodies, because the liquid state is always preceded by softening. It is probable that common glass and crystallized glass (Reaumur's porcelain) possess two different fusing-points.—*Ann. de Chim. et de Phys.*, Juin 1842.

EQUIVALENTS OF CERTAIN ELEMENTARY BODIES. BY M. DUMAS.

"*Composition of Water.*—Water is continually forming and decomposing by animals and plants; in order to appreciate what results, let us first examine what its composition is. Very delicate and difficult experiments, founded on the direct combustion of hydrogen, and in which I produced more than two pounds of water, the errors of which are unimportant under present circumstances, render it very probable that water is composed of

1 part of hydrogen, and
8 parts of oxygen,

and that these whole and simple numbers express the true proportions in which these two elements combine to form water.

"As bodies always present themselves to the eye of the chemist as molecules, and since he always endeavours to fix in his thoughts the weight of the molecules of each substance, the simplicity of these relations is not unimportant. In fact, each molecule of water being found to consist of a molecule of hydrogen and a molecule of oxygen, these simple numbers are learnt and are never forgotten.

"A molecule of hydrogen weighs 1, a molecule of oxygen weighs 8, and a molecule of water weighs 9.

"*Composition of Carbonic Acid.*—Carbonic acid is incessantly produced in animals, and incessantly decomposed by plants; its composition therefore merits in its turn special attention.

"Experiments founded on the direct combustion of the diamond, and its conversion into carbonic acid, have proved to me that this acid is formed of 6 parts by weight of carbon and 16 parts by weight of oxygen.

"We are therefore led to represent carbonic acid as being formed of a molecule of carbon weighing 6, and 2 molecules of oxygen weighing 16, which constitute a molecule of carbonic acid weighing 22.

"Carbonic acid then, like water, is represented by the most simple numbers.

"*Composition of Ammonia.*—Lastly, ammonia seems also formed, in whole numbers, of 3 parts of hydrogen and 14 of azote, which may be represented by 3 molecules of hydrogen weighing 3, and 1 molecule of azote weighing 14.

"Thus, as if better to exhibit her power, nature in organized substances always acts upon a very small number of elements combined in the most simple proportions.

"The whole atomic system of the physiologist turns upon these four numbers, 1, 6, 7, 8.

1 is the molecule of hydrogen;
6 that of carbon;
7 or twice 7, that is to say 14, that of azote;
8 that of oxygen.

Let the chemist always attach these numbers to these names, and there will no longer exist for him either hydrogen, carbon, azote, or oxygen in the abstract. These are substances the reality of which he has always seen; it is of their molecules that he always speaks, and to him the word hydrogen means a molecule which weighs 1, the word carbon a molecule which weighs 6, and the word oxygen a molecule which weighs 8."—*Essai de Statique Chimique des Êtres organisés*, 1842.

ON SANGUINARINA. BY M. SCHIEL.

According to M. Dana, sanguinarina is extracted from the root of the *Sanguinaria Canadensis* by treating it with anhydrous alcohol, adding water and ammonia to the solution, then washing the red precipitate formed with acid, and boiling it in water and animal charcoal. After pouring off the water, the mixture of the base and charcoal is to be treated with alcohol, and the solution being evaporated, the base remains in the state of a mass of a pearl-gray colour. M. Schiel prefers in preparing this substance the process adopted by Probst for that of *chelérythrina*, procured from the *Chelidonium majus*, the appearance of which resembles that of the sanguinarina.

The dried and powdered root is to be treated with æther; the solution is to be filtered and a current of hydrochloric acid gas is to be passed into it. This acid occasions the precipitation of impure hydrochlorate of sanguinarina, which is separated by filtration; this being dried by a gentle heat is to be dissolved in hot water, and excess of ammonia added to the solution. The precipitate, which is then formed, is washed upon the filter and then dissolved in æther. The solution is to be shaken with blood-charcoal recently calcined, until, on depositing the charcoal, it appears quite colourless; on passing hydrochloric acid gas into the solution, a precipitate of pure

hydrochlorate of sanguinarina of a magnificent scarlet colour is obtained. This salt, when dissolved in water, yields, on the addition of ammonia, pure sanguinarina in white or slightly coloured flocks, which become a yellow powder by washing and drying.

Sanguinarina is insipid, occasions violent sneezing, and soon becomes red in an atmosphere which contains even a small quantity of acid vapours. It is insoluble in water, very soluble in alcohol and æther; the alcoholic solution has a very bitter taste, and a manifestly alkaline reaction. When heated it fuses, and has the appearance of an oil, and when burnt it leaves no residue. It neutralizes acids perfectly and forms red salts with them, which are very soluble in water, and possess very decided bitterness. Chloride of platina precipitates them of an orange-red colour, and infusion of galls of a yellowish-red. Concentrated nitric acid decomposes sanguinarina, and when dried at 212° it consists of

Carbon	70·03
Hydrogen	5·27
Azote	5·23
Oxygen.......... ...	19·47
	100·

The following formula is that which best agrees with the results of analysis:—

37 atoms of carbon	2806·45	or	70·62
32 ... hydrogen	190·00	...	4·78
2 ... azote	177·03	...	4·45
8 ... oxygen	800·00	...	20·15
	3973·48		100·

Hydrochlorate of Sanguinarina.—This salt, obtained by the process already indicated, is a red, agglutinated, friable mass; the powder, when examined by a microscope, appears to be an agglomeration of well-defined small crystals. This salt is very soluble in water and in alcohol, especially when heated, but it is insoluble in æther.—*Journal de Pharm. et de Chimie*, Novembre 1842.

[We may take this as a suitable opportunity of remarking how much more simple and easy of application are the whole numbers which have been long used by many chemists in this country to represent equivalents than those generally employed on the continent; and we trust that the able support given to the doctrine of whole numbers by M. Dumas, as quoted in our present Number, will have its proper weight both here and abroad. If we reckon that sanguinarina is constituted of equivalents represented by whole numbers, we shall have its composition as under:—

37 eqs. of carbon....	$6 \times 37 =$	222	or	70·25
16 ... hydrogen..	$1 \times 16 =$	16	..	5·06
1 ... azote	$=$	14	..	4·43
8 ... oxygen ..	$8 \times 8 =$	64	..	20·26
		316		100·

It will be observed that we have here 316 instead of 3973·48 representing an equivalent of sanguinarina; in confirmation of the pro-

bable greater accuracy of the whole numbers we may observe, that if we compare the results of the analysis of 100 parts with that of the composition of 100 parts calculated according to the different equivalent weights, the result is very considerably in favour of whole numbers.

				Difference.
Carbon	by analysis		70·62	
...	...	fractional numbers	70·03	0·59
...	...	whole numbers	70·25	0·37
Hydrogen	by analysis		5·27	
...	...	fractional numbers	4·78	0·49
...	...	whole numbers	5·06	0·21
Azote	by analysis		5·23	
...	...	fractional numbers	4·45	0·78
...	...	whole numbers	4·43	0·80
Oxygen	by analysis		19·47	
...	...	fractional numbers	20·15	0·68
...	...	whole numbers	20·26	0·78

EDIT.]

METEOROLOGICAL OBSERVATIONS FOR NOVEMBER 1842.

Chiswick.—Nov. 1. Cloudless and very fine: foggy at night. 2. Foggy. 3. Hazy. 4. Cloudy: slight showers. 5. Overcast: sleet. 6. Slight showers: cloudy. 7, 8. Cloudy. 9. Densely overcast: stormy, with rain at night. 10, 11. Rain. 12. Stormy and wet: clear at night. 13. Boisterous, with rain. 14. Overcast: very fine: boisterous, with heavy rain at night. 15. Stormy and wet: foggy. 16. Rain: drizzly: clear at night. 17, 18. Overcast. 19. Heavy rain. 20. Overcast. 21. Clear. 22. Rain, with some sleet. 23. Lightly overcast: rain. 24. Fine: lightning, with rain at night. 25. Heavy rain. 26. Clear and fine. 27. Fine: stormy, with rain at night. 28. Cloudy: rain: fine. 29, 30. Very fine.—Mean temperature of the month 0°·18 above the average.

Boston.—Nov. 1. Fine. 2. Foggy. 3. Cloudy. 4. Fine: rain early A.M.: hail and rain P.M. 5. Fine: rain P.M. 6—8. Fine. 9. Windy: rain P.M. 10. Cloudy: rain P.M. 11. Stormy: rain early A.M.: rain P.M. 12. Fine. 13. Cloudy: rain A.M. and P.M. 14. Cloudy. 15. Cloudy: rain early A.M. 16. Cloudy: rain A.M. and P.M. 17. Fine. 18. Cloudy. 19. Rain. 20, 21. Fine. 22. Rain: rain early A.M.: snow P.M. 23. Fine: rain P.M. 24. Rain: rain early A.M. 25. Fine. 26, 27. Fine: rain P.M. 28. Rain: rain early A.M.: rain P.M. 29. Fine. 30. Cloudy: rain early A.M.

Sandwick Manse, Orkney.—Nov. 1. Drizzle. 2. Cloudy. 3. Cloudy: clear: aurora. 4. Very clear: aurora. 5, 6. Cloudy. 7. Sleet-showers. 8. Damp: rain. 9. Rain: showers. 10. Sleet-showers: cloudy. 11. Damp: rain. 12. Rain. 13. Rain: showers. 14. Snow-showers: sleet-showers. 15. Frost and a little snow: clear and frosty. 16, 17. Frost: cloudy and frosty. 18. Cloudy: drizzle. 19. Drizzle: damp. 20. Drops: clear. 21. Hail-showers: clear: frost. 22. Cloudy: rain. 23. Cloudy: drops. 24—27. Showers. 28. Showers: a gale and rain. 29. Clear. 30. Cloudy: clear.

Applegarth Manse, Dumfries-shire.—Nov. 1. Fair and fine. 2, 3. Fair but dull. 4. Fair but dull: clear. 5. Fair but dull: hoar-frost A.M. 6. Fair but dull: a few drops. 7. Fair but dull. 8. Slight shower. 9. Heavy rain. 10. Frost A.M.: rain P.M. 11. Heavy rain. 12. Wet A.M.: cleared and fine. 13. Fair: hoar-frost A.M. 14. Fair and fine: frost P.M. 15. Fair, but raw and cold. 16. Fair and keen. 17. Fair but cloudy. 18. A few drops of rain. 19. Wet morning. 20. Fair, but raw and cloudy. 21. Fair: frost A.M. 22. Snow: frost: rain P.M. 23. Fair and fine: slight frost. 24. Rain and wind. 25, 26. Rain. 27. Wet morning: cleared. 28. Storm of wind and rain. 29. Slight showers. 30. Fair and fine.

Meteorological Observations made at the Apartments of the Royal Society, LONDON, *by the Assistant Secretary*, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *by the* Rev. W. Dunbar, *at Applegarth Manse*, DUMFRIES-SHIRE; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.								Thermometer.											Wind.					Rain.					Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		London: R.S.			Chiswick.		Boston. 8½ a.m.	Dumf.-shire.		Orkney, Sandwick.		London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond. R.S. 9 a.m.
1842. Nov.		Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Fahr. 9 a.m.	Self-reg. Mx.	Self-reg. Mn.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.											
1.	30·231	30·216	30·114	29·60	30·08	30·00	30·10	30·22	44·9	53·4	43·2	55	32	46	55	46	50	47	ssw.	w.	calm	sw.	nw.	......	......	......	......	·08	43
● 2.	30·140	30·126	30·084	29·64	30·05	30·01	30·24	30·22	48·3	52·6	43·8	51	29	48	51½	45	46	45	e.	e.	calm	e.	e.	......	......	......	......	......	45
3.	30·000	29·983	29·934	29·53	29·98	30·01	30·20	30·28	43·3	52·0	42·0	50	34	45	49	38	15	38	n.	e.	calm	e.	e.	......	·03	......	......	......	42
4.	30·060	30·169	30·073	29·67	30·16	30·28	30·44	30·54	39·5	49·0	37·8	42	31	40	47	35	38	36	nnw.	ne.	calm	e.	se.	......	·01	·10	......	......	38
5.	30·190	30·177	30·104	29·80	30·30	30·26	30·53	30·51	12·0	43·4	37·4	46	32	41	46	34	40½	43	n.	ne.	calm	nne.	n.	·033	·01	·17	......	......	37
6.	30·202	30·148	30·122	29·87	30·22	30·25	30·49	30·43	38·3	46·8	36·7	47	36	38	47½	37½	41½	43	n.	ne.	calm	n.	nw.	......	·01	·10	0·02	......	36
7.	30·130	30·131	30·099	29·88	30·21	30·10	30·30	30·00	42·3	43·6	38·4	48	38	42	46½	37	42½	43	nw.	ne.	calm	ne.	nw.	......	·01	......	......	·04	39
8.	30·152	30·126	29·959	29·75	29·92	29·63	29·83	29·42	42·3	48·2	40·6	46	38	38·5	48½	37½	13	45	nw.	nw.	calm	wnw.	w.	......	......	......	......	·16	39
9.	29·806	29·790	29·634	29·32	29·19	29·25	28·70	28·90	43·7	45·7	41·8	49	44	44	50½	42½	50	45	s.	sw.	w.	wsw.	sw.	......	·04	......	......	·50	37
☽ 10.	29·684	29·651	29·494	29·20	29·46	29·30	29·29	29·50	48·4	49·8	44·0	50	41	48·5	45	30	39	40	s.	s.	calm	ese.	w.	·038	·03	·12	......	·17	45
11.	29·180	29·548	29·089	28·74	28·97	28·85	29·37	29·12	50·7	52·3	43·2	55	47	44	45	35½	38½	43	s.	s.	s.	ese.	w by n	·019	·37	·08	......	......	45
12.	29·122	29·516	29·050	28·62	28·82	29·08	29·05	29·14	51·5	55·0	49·5	55	45	47	49	42	43	42½	w.	w.	calm	wsw.	e.	·261	·40	·50	......	·19	48
13.	29·512	29·457	29·284	28·97	29·15	29·20	29·26	29·44	50·0	55·5	48·0	54	39	48	47	39½	40½	38	sse.	sw.	calm	wsw.	ne.	·494	·29	......	......	·45	47
14.	29·720	29·763	29·675	29·19	29·62	29·72	29·67	29·86	47·7	55·0	46·7	53	43	47·5	44	35½	36	36	w.	e.	calm	nne.	n.	·266	·67	·55	1·05	·10	47
15.	29·686	29·631	29·614	29·33	29·68	29·68	29·97	30·05	43·3	51·6	43·7	46	44	44	44	27½	33	34	e.	e.	calm	ne.	n.	·625	·32	·15	......	·15	45
16.	29·762	29·896	29·717	29·45	29·78	30·00	30·15	30·30	44·0	47·0	43·6	45	37	43·5	42	36½	35	35	ne.	e.	e.	ene.	ne.	·355	·02	·21	......	......	43
17.	30·232	30·451	30·182	29·85	30·24	30·28	30·38	30·32	40·7	45·3	39·2	45	30	40	44	35½	34½	36½	nne.	ne.	calm	ese.	ne.	·033	......	......	......	......	37
○ 18.	30·582	30·532	30·412	30·13	30·29	30·15	30·16	30·21	41·3	45·0	40·2	45	26	38·5	44	37	46	40½	e.	s.	calm	e.	s.	......	......	......	......	......	37
19.	30·232	30·100	29·732	29·76	29·71	29·61	29·90	29·93	43·8	45·3	38·4	52	45	40	49	34	41	41	s.	sw.	sw.	sse.	w.	......	·87	·32	......	·05	41
20.	29·788	29·778	29·717	29·39	29·73	29·75	29·97	29·95	46·7	52·8	37·8	49	34	45	46	38½	36½	33	n.	ne.	calm	ne	e.	·677	......	·18	......	......	44
21.	29·800	29·799	29·677	29·44	29·79	29·66	29·95	29·77	40·3	49·6	40·3	46	31	37·5	39	29½	34	29½	n.	ne.	calm	e.	n.	......	·32	......	......	·05	38
22.	29·420	29·409	29·268	29·11	29·23	29·15	29·47	29·39	35·7	45·3	36·4	39	30	36	36	32½	35	36½	se.	e.	se.	e.	n.	·244	·17	·10	0·80	·02	37
23.	29·496	29·485	29·152	29·10	29·18	29·05	29·36	29·30	38·4	39·4	35·6	47	39	36	39	30	36½	37	s.	sw.	calm	se.	e.	·177	·11	·25	......	·11	37
24.	28·908	28·890	28·793	28·51	28·77	28·65	29·11	29·08	43·7	46·3	38·7	50	40	42	43	31	37	42½	s.	sw.	calm	e.	e.	·105	·32	·20	......	·12	40
☾ 25.	28·900	28·988	28·816	28·51	28·63	28·68	29·04	29·10	44·2	48·2	42·8	48	39	41	46½	40½	41½	42	s. var.	s.	calm	e.	e.	·300	·12	·05	......	·26	42
26.	29·094	29·208	29·060	28·63	28·74	28·90	29·10	29·10	43·4	47·6	41·3	50	29	38	43	38	43	43	sse.	sw.	calm	e.	e.	·105	·01	......	......	·16	40
27.	29·366	29·344	29·069	28·96	29·09	29·12	29·22	29·30	42·7	49·0	40·2	50	43	37	42½	36½	41½	38	s.	s.	calm	ese.	e.	......	·24	·02	......	·29	40
28.	28·944	29·266	28·893	28·55	28·73	28·83	29·18	28·88	49·3	50·8	43·0	53	40	47	50	37	43	44½	se.	s.	s.	sse.	e.	·222	·04	·50	......	·10	45
29.	29·540	29·527	29·519	29·02	29·08	29·27	29·09	29·26	44·4	54·3	44·2	53	39	41	49½	39	40	42	se.	sw.	calm	s.	se.	·061	·01	·10	0·90	·81	42
30.	29·662	29·975	29·602	29·22	29·44	29·67	29·45	29·70	46·2	51·2	44·6	52	29	43·5	46½	39	41½	40	w.	w.	calm	s.	s.	·033	·01	·10	......	......	44
Mean.	29·718	29·776	29·597	29·29	29·541	29·549	29·699	29·707	44·0	49·0	41·4	49·03	36·80	42·2	45·8	36·5	40·41	39·91						Sum. 4·018	4·47	3·80	2·77	3·88	Mean. 41

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XIII. *On the Constitution of the Sidereal System, of which the Sun forms a part.* By O. F. MOSSOTTI, *Professor of Pure and Applied Mathematics in the University of the Ionian Islands**.

ASTRONOMY, as if sensible of the smallness of the being that was creating it, was very slow and backward in discovering the immensity of the field that lay open to its investigations. The first astronomers, believing the earth to be immoveable in the centre of the universe, did not dare to extend the limits of the heavenly vault beyond a million of geographical miles. When the Pythagorean school gave forth the bold conception, that the earth was a planet revolving round the sun, it became necessary to consider the radius of the earth's orbit as of insensible magnitude with regard to that of the celestial sphere, and Aristarchus of Samos (who had adopted the ideas of the Pythagorean school) increased the radius of the latter six hundred and thirty-five times†. The limits of the universe are however infinitely more remote: the depth of the heavens confounds itself with the immensity of space. In the last century astronomers determined with precision the distance that separates us from the sun, and, what is no less wonderful, the rapid velocity with which light is propagated. The eighty-two millions and two-thirds of a million of miles between the earth and the sun are traversed by light in the short time of eight minutes and thirteen seconds. Now, according to the recent accurate calculations

* Extracted from an Introductory Lecture delivered by the Author on the 1st of October 1839, at the opening of the University Session: printed at Corfu in 1840. Translated from the Italian, and communicated at the request of the Author, by E. H. J. Craufurd, B.A., Trin. Coll. Cambridge.

† Arist. Sam. *de magnit. et dist. Solis et Lunæ*. Edit. 1572. in 4to. Pappus *Coll. Mathem.*, lib. vi. prop. 38. *Archimedes in Arenario.*

of M. Bessel, ten years and a quarter would scarcely be sufficient for the light from one of the stars which we may suppose the nearest, viz. 61 Cygni, to reach us. And, if we consider the smallest stars visible to the naked eye as those which are placed at an intermediate distance between the nearest and the most remote or telescopic stars, we may presume with Sir J. Herschel, without fear of departing greatly from the truth, that the light from many of them takes some thousand years in reaching us: so that, to avail myself of the expressive style of that writer, when we observe those stars, when we note their changes, we are reading and writing their history of a thousand years ago. Yet distant as they may seem, these stars do not mark the limits of the universe; they are only those that constitute our sidereal system. There are in the heavens groups of stars and divisible nebulæ, which, according to all appearance, form separate sidereal systems. The distances at which these systems are placed must be as much greater than those of the common stars, as the distances of these are greater than the dimensions of our planetary system. Following out the increasing progression of these intervals, our imagination fails, and is bewildered in the conception of such an immensity of space.

I have wished, Gentlemen, to recall to your memory these notions of the heavenly distances, to prepare your minds, and introduce them to the vast theatre in which occur the phænomena that are to form the subject of my discourse. I do not, however, intend to speak of the remotest sidereal systems, which are scarcely discoverable by the most powerful telescopes; they are too distant, and as yet we know too little of them. My lecture will treat of the constitution of the sidereal system of which our sun forms a part, of the form according to which it has been fashioned, and of the mechanical conditions which ensure its stability during the lapse of centuries.

No one who has turned his eye to the heavens, but must have dwelt with wonder on that streak of light of an irregular whiteness which surrounds the heavenly vault like a belt. This belt has received the name of *Galaxy*, or *Milky Way*, ever since poets described it as produced by Juno's milk which the infant Hercules had let drop from his mouth. An ancient philosopher, Metrodorus, believed that the Milky Way was the sun's path, and that the luminary having on its passage kindled the stars with its heat, that portion of the heavens remained of a whitish colour owing to their scattered ashes*.

* Plutarch. *de Placitis,* lib. iii. cap. i.; and Manilius, *Astronomicon,* lib. i. vers. 727. *et seq.*

Œnopidas of Chios also asserted that he had learnt from the Egyptian priests that the sun moved in the Milky Way, and then, mistaking his apparent annual motion for his real motion, added with poetic fiction, that the sun, horror-struck at the sight of the banquet of Thyestes, turned from his course, and revolved ever afterwards in the ecliptic *. The essence and the structure of this part of the heavens remained uncertain up to the present time: to this the great poet of the *Divina Commedia* alluded when he said,—

> "Come distinta da minori e maggi
> Lumi biancheggia fra i poli del mondo
> Galassia sì che fa dubbiar ben saggi."
>
> *Parad.* canto xiv. ver. 97.

> "As leads the galaxy from pole to pole,
> Distinguish'd into greater lights and less,
> Its pathway which the wisest fail to spell."
>
> *Cary's Translation*, verse 90.

It was the celebrated astronomer Sir W. Herschel who, penetrating it with his powerful telescopes, analysed its formation. He saw the light, which was concentrated in distinct points, present the appearance of an immense number of stars, so that he was able to conclude that more than 50,000 stars had passed over the field of his telescope in a zone only two degrees in breadth during the short space of one hour. Thus was completely verified the conjecture, already put forth by Democritus, that that light was in the greatest part the effect of the concourse of a multitude of stars too minute or too remote to be distinctly perceived.

The more Herschel was aided by his instruments, which were wrought to so high a degree of perfection by his genius, the more acute he showed himself in drawing his conclusions. He supposed then that the Milky Way was a cluster of stars, a resolvable nebula in the form of a stratum, of which the thickness, although immense, was yet very small in comparison with its other dimensions, and in which the sun and his system of planets were placed. As any person who stands in the midst of a low stratum of thin and transparent haze perceives it above and close to him, while it appears thicker and darker in the distance, until it assumes the form of a circle of greyish light along the horizon, so an observer who is placed in the milky nebula must see the stars thinly scattered if he looks around him; whereas if he directs his visual ray along the plane of the stratum he will see them thickly condensed: and if with his eye directed along that plane he sweeps from one end of the sky

* *Vide* cap. xxiv. of the Introduction to the Phænomena of Aratus, written by Achilles Tatius, and inserted in the *Uranologium* of Père Piteau.

to the other, a circle of pale confused light will appear to be projected or painted on the vault of the heavens.

The structure of the sidereal system in which we are placed, according to the idea we have just formed of it, agrees as far as appearances go with the principles of geometry and perspective, but it is necessary besides to verify whether this structure is in accordance with the laws of mechanics, with the conditions of a system possessing the property of preserving itself unaltered in the course of time. The attribute of long conservation is a character which the Author has impressed on all the wondrous bodies of the heavens:

"Cœlestia semper
Inconcussa suo volvuntur sidera lapsu *."

Analogy leads us to believe that the Newtonian law of attraction which acts between all material particles of which the bodies in our planetary system are formed, obtains equally among those of all other bodies scattered through the universe: the proofs of the legitimacy of this induction by analogy we shall borrow from Herschel himself. We find in the heavens stars which appear single when seen by the naked eye, but which when examined with a telescope are found to consist of two or more stars, and which therefore are called *double* or *multiple stars.* In order that this apparent coincidence of two stars may take place, it is sufficient that they be placed very nearly in the direction of the same visual ray, but one of the stars may be thus situated at a much greater distance from us than the other. Now Herschel having observed several of these double stars, in order to determine the annual parallax of one of them, met with an unexpected phænomenon. He remarked that in the greater number of these double stars, one star, in the course of years, revolved round the other; or to speak more precisely, both revolved round their common centre of gravity. Each of these double stars constitutes therefore a peculiar system; the two stars are not approximately on the same visual ray, but are really close to one another and attract each other powerfully, and the calculation of their motions affords sufficient proof that they obey the Newtonian law. The existence of a mutual attraction between the stars being thus proved, we necessarily conclude that they move; but a system of motions which shall remain unaltered in a long series of revolutions, does not appear reconcileable in a system of bodies, the masses of which are promiscuously large and small, attracting one another in the inverse ratio of the squares of the distances and situated in

* Lucan, *Phars.*, lib. ii. 267.

one plane. The structure then of the Milky Way which we have been considering requires some modification.

This modification is suggested by an observation of Sir J. Herschel, the son of the above-named, who, inheriting the genius of his father and trained by an excellent scientific education, is now one of the luminaries of British science. This astronomer, while residing at the Cape of Good Hope, wrote to a distinguished Italian mathematician, Commendator Plana, nearly in the following terms:—" A circumstance, with regard to the structure of the Milky Way, which forcibly strikes me every time I observe the heavens during any of these serene and clear nights, as frequent here in summer as they are in your lovely Italy, is that the portion of this wonderful zone, which lies between Sirius and Antares, is perfectly illuminated by the stars, of which a multitude is visible to the naked eye. If starting from the centre of this portion we follow its direction with the eye towards the north, the illumination gradually diminishes until nothing remains but a weak hazy light without any trace whatsoever of stars. The southern half of the Milky Way appears therefore nearer to our solar system than the northern half, or in other words, the Milky Way is not merely a stratum, but a ring of stars in which the sun is situated excentrically, being nearer to the constellation of the Cross than to the point diametrically opposite."

You will allow me here to recall to you, that the constellation of the Cross, of which mention is here made, is the same that Dante has rendered remarkable by an assertion to which too absolute a sense has been given in these lines:—

> " Io mi volsi a man destra, e posi mente
> All' altro polo; e vidi quattro stelle
> Non viste mai, fuor ch' alla prima gente *."
> *Purg.* canto i. ver. 22.

> " To the right hand I turn'd, and fix'd my mind
> On the other pole attentive; when I saw
> Four stars ne'er seen before, save by the ken
> Of our first parents." *Cary's Translation,* verse 22.

The ingenious observation of Sir J. Herschel gives to our notion of the milky nebula that completeness which his father, who was not acquainted with the southern hemisphere, was unable to give. The milky nebula, in accordance with its appearance, has therefore the shape of a ring, such as would

* In the notes commonly appended to the *Divina Commedia*, the question is frequently mooted, in what way Dante could be informed of the principal stars which now form part of the constellation of the Cross, and it is supposed that he may have obtained such information from Marco Polo, who had crossed the equinoctial line in his travels. This may have

be formed by cutting out the internal portion of a disc, and the sun is situated near its internal edge.

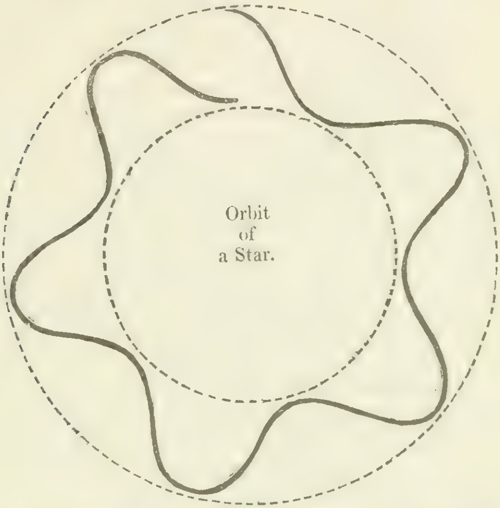
I would now entreat you, Gentlemen, to lend me your attention while I proceed to examine whether this form of nebula agrees with the mechanical conditions of a permanent preservation. According to the beautiful theory which the celebrated Laplace has given of the attraction of Saturn's ring, and according to the doctrine which Professor Plana has set forth in the 24th volume of the *Memorie dell'Academia di Torino*, on the attraction of bodies of various shapes, we conclude that if a body is situated near either the interior or the exterior edge of a plane ring, the body is attracted towards a point situated nearly in the centre of the thickness of the ring. If then we consider the resultant of the innumerable attractions of the stars distributed in the annular space of the Milky Way as the same with that of a continuous ring, it follows that a star placed in the internal edge of the Milky Way will tend to penetrate into the adjacent annular space, near the centre of which it would find its position of equilibrium. On reaching this central point, however, with the velocity generated in its motion, it will not stop there, but will proceed by the law of inertia towards the external edge; as soon as the velocity of the star is destroyed by the force of attraction, which acts now in the opposite direction to that of its motion, it will change its path, and will again pass through the centre of the annular space with a certain velocity, and will proceed towards the internal edge, to the place whence it first started. Having returned here, it will tend to repeat another similar oscillatory motion, then a third, and will thus go successively oscillating between the two edges of the plane of the annular space.

Let us suppose now that the star is projected with a certain velocity in the direction of the tangent at the point of the curve in which it is situated; and let us consider the resultant motion arising from the motion thus impressed and from the oscillatory motion above described. According to the principles of mechanics, it appears that the velocity of projection may be such as to make the star move repeatedly round over the

been the case; but Dante's verses must not be taken in an absolutely literal sense. The constellation of the Cross has about 30° south polar distance; it is visible and rises several degrees above the horizon in Hindostan and in Arabia, as for instance at Mecca: and the existence of the principal stars which form it, although commonly unknown, could not be so entirely, and especially to such as were acquainted with astronomy, as Dante was. In fact, the stars of which this poet speaks, and of which Royer in 1679 formed the constellation of the Cross, are found even in Ptolemy's Catalogue, though not exactly determined in position.

whole annular space, crossing it in its path alternately towards the outer and the inner edge, thus describing a wavy line and revolving in an infinite number of revolutions, all limited within that space. What wehave said of one star is equally appli-

Fig. 1.



cable to all of them, even though they were moving in a given annular space with given velocities*. The countless stars of

* I have not entered into any calculations, since the want of sufficient data at present would render them too hypothetical. It needs, however, but little reflection to see that the velocity of a star will be least when it approaches the external edge of the ring at the moment of changing its direction. If then we conceive several stars distributed along the orbit described by one of them, all proceeding in the same direction, they will approach each other more closely towards the outer edge than they will towards the inner edge. This greater crowding of stars towards the external edge being the same all round the ring, will make the attraction there vary more rapidly, and hence a star will be sooner retarded in its motion, and made to recede, when it is proceeding from the centre of the annular space towards the exterior edge, than when it is moving from that centre towards the interior edge. The stars therefore in their course will go further from the circumference of equilibrium, that is, from the circumference in which the radial force is zero, towards the interior than towards the exterior edge: thus the centrifugal force which tends to carry the stars from the centre of equilibrium will be counteracted, and the ring will preserve its dimensions permanently.

If we consider the symmetry of an annular system, and apply to it the general formulæ of the principles of *vis viva* and of the *conservation of areas*, in the forms given by Lagrange in the volume of the Memoirs of the Academy of Berlin for 1777, we shall easily see that there are many cases in which the velocities of all bodies must be confined within certain limits, and hence that a system of attracting bodies in the shape of a ring can preserve a permanent form, or at most will be subject merely to a tremulous motion.

the Milky Way may therefore constitute an unchangeable system, circulating in an annular space to which they are always limited, but to the internal or external circumference of which they will approach alternately in their motion.

The interesting result lately published by Mr. Argelander of his researches on the direction of the motion of the sun, affords a confirmation of the possibility that the motion of that luminary and of his planets should be such as we have described it. The first attempt to discover the sun's motion is also due to Sir W. Herschel. When we reflect on the immensity of the dimensions of the orbits, on the length of the periods required for completing the revolutions of the stars; when we consider that the circular movement of the ring is probably in the same direction with respect to all the stars, and that it is only their relative motion that we can more immediately perceive, it becomes evident that a long course of centuries is required to manifest the change of position of our solar system; notwithstanding this, Sir W. Herschel, ever since 1783, had indicated the star λ of the constellation Hercules as the point to which the sun tends in his motion. Against this fact many objections had been raised which rendered it less positive: but the uncertainty which still remained was finally cleared up by the above-named distinguished astronomer, late Director of the Observatory at Abo. For having subjected this question of sidereal astronomy to a more accurate calculation, by the discussion of an increased number of observations, he discovered that our solar system is moving towards a point of the constellation Hercules near the star 143 of the 17th hour according to Piazzi's Catalogue, which point differs little from the one noted by Herschel. The sun being at present, as we observed, in a point of the internal edge of the Milky Way, just at the moment of ceasing to approach to it, or rather in the act of receding from it, must in this deflection of its path have a direction of motion nearly in the tangent to the curve which forms the internal limit of the annular space at that point. Now, by a remarkable coincidence, if from the constellation of the Cross we draw a tangent to the circle of the Milky Way in a direction slightly elevated towards the northern hemisphere, we find that it must pass near the above points of the constellation Hercules. The solar system revolves, therefore, in the Milky Way round the centre of gravity of this mass of stars; and with an analogy which we must not omit to notice, proceeds round this centre from west to east, exactly in the direction in which all the bodies of this system revolve, the greater round the lesser.

To give in a few words a clear image of what has been said, consider a cluster of countless stars in the immensity of space,

all placed along a plane* ring of enormous dimensions, and all moving in it in periods which only myriads of centuries can measure; following them in their long and slow courses, imagine them to approach promiscuously but alternately the outer and inner edge of the ring, and you will have an idea of the sidereal system in which we are placed, such as I have conceived it, and such as I have wished to show it you in this discourse.

Addenda.—I. The distinguished Mr. Henderson, Director of the Edinburgh Observatory, communicated to the Royal Astronomical Society of London, at the beginning of the present year, the result of the observations which he and Lieut. Meadows have made at the Cape of Good Hope, on the double star $\alpha' \alpha'$ of the constellation of the Centaur. This result shows that that star, which has a very sensible motion of its own, has an annual parallax of about one second, that is, three times as great as that which M. Bessel found for the double star 61 Cygni, so that the former star must be three times as near to us as the latter. The double star $\alpha' \alpha'$ of the Centaur is projected on the edge of the Milky Way on the side of the constellation of the Cross, that is, in that part of the ring in which we have said our solar system is at present situated; and according to what we have shown it is clear that it is on this very side that we should have a greater chance of meeting with stars nearer to us. In general, the fact that stars which are more visible and which have a more sensible motion of their own, such as Bootes, Sirius, Procyon, are found in that hemisphere of the heavens in which the segment of the ring of the Milky Nebula which is nearest to us is situated, is a circumstance that tends to confirm the constitution of the sidereal system which we have attempted to explain in the foregoing discourse.

* Although we have always spoken of a plane ring, yet to satisfy all appearances we must suppose it of a slightly conical form, i. e. we must suppose it to have the shape of a portion of the surface of a very obtuse right cone intercepted between two planes parallel to the base. For if the ring were plane, the Milky Way would appear narrower in the neighbourhood of the constellation Cassiopeia, viz. in the part opposite that in which we are placed, than on our side, viz. in the neighbourhood of the constellation of the Cross; whereas, in fact, in the former, though it sheds a weaker light, it has the appearance of greater width. This can be explained by assuming the ring of the above-mentioned shape. To an observer situated near the interior edge of the ring on the side of the constellation of the Cross, the Milky Way on this side would appear of a thickness corresponding to the angle contained between two visual rays, which, penetrating to a certain distance between the stars in the ring, embrace its thickness, while on the opposite side, besides the thickness, a portion of the internal surface would be visible in a very oblique direction, and this would give to the Milky Way the appearance of a greater width.

II. As the Cavalier Carlini, the renowned astronomer of the Observatory at Milan, has pointed out to me some parts of the foregoing discourse, which, unless more fully developed, seem to leave some difficulties in the hypothesis of the sidereal system set forth in it, I avail myself of this opportunity to explain them.

The first point is the modification, which, without any reason being adduced, I have apparently given to the ideas set forth by Sir J. F. W. Herschel, by placing the sun on the internal edge of the ring, whereas the passage I have quoted seems to allude to the sun as being placed excentrically within the ring rather than as forming a constituent part of it. It is however easy to see that the distinguished philosopher in those few lines wished merely to throw a luminous idea on the cause which might produce the appearance of a difference in the brightness of the two portions of the Milky Way, without having any other object in view. When we bring into consideration also the circumstances of the motion produced by a mutual attraction, it is evident that all the bodies in the interior of the ring are brought to form a part of the ring, and to cross it from time to time, and this must be the case with our sun also. It is probably in moving across the ring that the temperature of the solar system is greatly increased, according to the plausible hypothesis by which M. Poisson explains the internal heat of the terrestrial globe, and the geological changes to which it has been subjected. Some stars will depart more, others less from either side of the *circumference of equilibrium*, according to the places in which they were situated, and according to the direction and the velocity with which they were put in motion. Our sun is probably one of those that depart furthest from it, and descend further into the empty space within the ring.

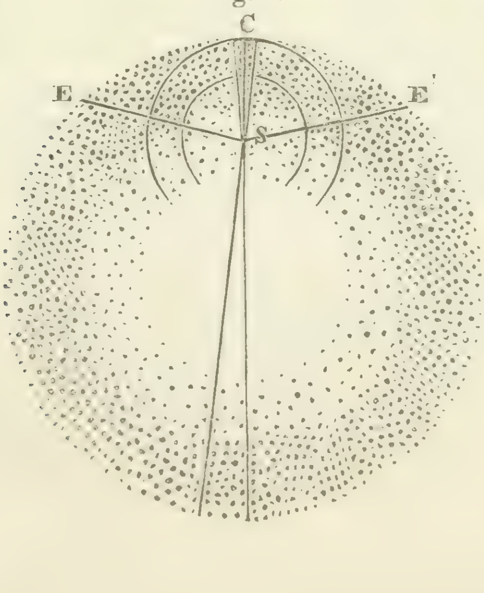
The annular-shaped system of stars of which we are speaking, is not singular in the heavens. Other nebulæ have been observed of this shape, and among others one very distinct situated between the stars β and γ Lyræ, and presenting in its appearances the very peculiarities which we have described in our system.

The second point is the circumstance, that if we are placed on the interior limit of the ring on the side where the constellation of the Cross is situated, the Milky Way ought to appear brighter, not in the direction of this constellation, but rather in the lateral directions, since in these directions the visual rays cross a greater extent of the thickness of the ring. But if attentively considered, this circumstance, instead of throwing any difficulty in the way, affords a new and not slight argument in favour of the assumed hypothesis.

I will premise, that, according to the impression I have retained of the heavens in the southern hemisphere, the Milky Way does indeed present in that portion a clearer and more distinct brightness, as if arising from stars nearer to us, but not such to all appearance as to be attributable to a number of stars greater than elsewhere, for instance, towards the constellation Scorpio. Now if we consider that, as has been said in the note (p. 87), the stars in the ring, by the conditions of their motion, necessarily crowd more towards the part without than towards the part within the circumference of equilibrium, it will be easy to conceive, as may be seen on inspection of fig. 2, that a greater quantity of stars, more distinct, and giving greater light, will be seen in the direction of the radius S C, normal to the arc of the ring nearest the solar system which we suppose in S, than in the direction of the oblique radii S E S E′; so that the absence of a greater degree of illumination on either side of the constellation of the Cross goes to confirm the mechanical conditions which, as we have observed, must subsist in the supposed system.

I must not omit to observe, as has been noticed above, that some of the stars will describe more, others less wavy orbits, i.e. some will depart more, others less from that circumference in the ring where they would be in equilibrium. The crowding of the stars will be greatest in the neighbourhood of this circumference, and will diminish gradually as they move from it, but more rapidly towards the interior than towards the exterior edge.

Fig. 2.



The third and last point relates to the difficulty which might arise from the consideration, that if the ring is of such enormous dimensions, and if the portion of it opposite that in which we are placed is so very far from us as we must suppose it to be, how does it happen that the light of that portion is not much less than that of the part nearest to us? To account for this effect, let us suppose the point of the interior edge of the ring S, in which the solar system is placed, to be the common vertex of two opposite cones of small and equal vertical angles (fig. 2), each produced respectively to the outer edge of the ring. The visual angles under which the two opposite portions of the Milky Way intercepted by the two cones will be seen will be equal; that is to say, the two portions will appear equal: but a much greater number of stars will contribute to the illumination of the furthest portion, inasmuch as the section made by the cone is much greater at that distance, whereas a smaller number of stars will contribute to illuminate the nearest portion. The greater quantity of stars giving light will thus compensate to a certain extent for the greater distance, and the brightness of the Milky Way cannot differ a great deal in its different parts.

To this cause we must add, that, if the sun be not really on the edge of the ring, as probably it is not, but there be other stars between it and the empty space within the ring, they will necessarily tend to increase in a slight degree the brightness of that portion of the Milky Way which is opposite to the constellation of the Cross.

XIV. *On a new Experiment in Physical Optics.* *By the Rev.* S. Earnshaw, *M.A.*, *Cambridge**.

BY a train of reasoning confessedly obscure, but nevertheless founded essentially on a theory of *undulations*, Fresnel obtained the following formulæ for the amplitudes of the vibrations of light reflected at the surface of a medium of no double refraction, viz.

$$+ a \frac{\sin(i-i')}{\sin(i+i')} \dots \text{(A)} \quad \text{and} \quad - a \frac{\tan(i-i')}{\tan(i+i')} \dots \text{(B)}$$

the former (A) for light polarized in the plane of reflexion; and the latter (B) for light polarized at right angles to the plane of reflexion. It ought not to be reckoned an insuperable objection to the reception of these formulæ, were they not found to give the exact degree of brightness observed in experiments. More stress may however be laid upon the neces-

* Communicated by the Author.

sity of their exact agreement with experiment in those phænomena which depend upon the change of the algebraic signs, or upon the equality of intensity of the two kinds of light, or the evanescence of one of them. In all cases of this nature which have been examined, I believe the results have been found to be most satisfactory. The formula (A) is always positive while the angle of incidence changes from zero to 90° : (B) is negative at first and afterwards positive; vanishing entirely, in exact accordance with Brewster's law, at the intermediate stage of $i + i' = 90°$. All this is shown by the Astronomer Royal in the Cambridge Transactions, vol. iv., in a series of very interesting experiments, to be in perfect agreement with nature. In the extreme case of $i = 90°$, Professor Lloyd, in vol. xvii. of the Irish Transactions, by a very simple and elegant experiment, has proved that both (A) and (B) are rigidly exact, both as to their algebraic signs, and the relative magnitudes of the vibrations which they represent. In the other extreme case of $i = 0°$, I am not aware of any very satisfactory experiment of a direct nature, not depending on photometry, having been made. I beg therefore to propose one, which I believe is perfectly new, and of such a nature as to be free from any obscurity or doubt as to the phænomenon to be observed. It will be remarked that when i is very nearly 90°, the formulæ (A) and (B) become

$$+ a \frac{\mu - 1}{\mu + 1} \text{ and } - a . \frac{\mu - 1}{\mu + 1}.$$

Now these are exactly equal, but of opposite signs, indicating that both species of light are reflected with equal intensities; but while the phase of that portion whose vibrations are perpendicular to the plane of reflection is unaffected, the phase of that portion whose vibrations are in the plane of reflexion is accelerated or retarded by half an undulation. Consequently, if *right-handed* circularly polarized light be incident nearly perpendicularly upon a plane surface of glass, Fresnel's formulæ lead us to expect that the reflected light will be *left-handed* circularly polarized, and *vice versâ*. I trust that the simplicity of the experiment here proposed, as well as its newness, will induce some one of your experimental correspondents to undertake it. The result, I have no doubt, will be the addition of another instance to the list of fulfilled predictions of the undulatory theory.

Cambridge, Nov. 30, 1842.

XV. *On the coloured Rings produced by Iodine on Silver, with Remarks on the History of Photography.* *By* H. F. Talbot, *Esq., F.R.S., &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In your Number for December 1842, you have inserted an interesting account of some experiments by Dr. Waller on coloured films:—in which account, however, I have noticed with some surprise what is there stated to be a *new method* of making coloured rings, like those generally known under the name of "Newton's coloured rings," on many of the metals.

"In order to procure these coloured rings," says Dr. Waller, "we have but to place a piece of iodine on a well-polished surface of silver or copper, and in a short time we find around the iodine a series of coloured zones of the various tints of the spectrum." In the next page he adds, "the action of light on the different colours is very interesting; the most correct way of studying this, is to protect one half of a system of coloured rings by an opake screen, while the other half is exposed for a short time to the influence of the solar rays. The golden zone . . . . . . is converted into a beautiful green," &c. &c. &c.

Now, since the History of Photography will probably be written some day or other, it is desirable that the different phænomena discovered should be ascribed to their first observers, with as much attention to accuracy as possible.

As this is in most cases the only reward of scientific researches, justice requires that it should be scrupulously adhered to, and if by accident a mistake occurs it ought to be speedily rectified.

Give me leave therefore to state that this method of forming Newton's rings was first discovered and published by myself; and that I particularly called attention to the beautiful phænomenon which occurred when the rings were formed on *silver*, namely, that they were *sensitive to light*, and when held in the sunshine transmuted themselves into other colours,—a fact until then quite unexampled in Optics.

I brought forward the matter at the Birmingham Meeting of the British Association on the 26th of August, 1839, and a full report of it will be found in the Athenæum for that year, page 643, and in the Literary Gazette, page 546*.

From the ample publicity which I gave to it at that time,

* An abstract of Mr. Talbot's communication was afterwards given in the Report of the Ninth Meeting of the British Association, Transactions of the Sections, p. 3.—Ed.

I was in hopes it had become known to the scientific world; but as that appears not to be the case, will you allow me to occupy a page of your Journal with an extract or two from the above-mentioned report contained in the Athenæum?

After alluding to M. Daguerre's process (then just divulged) of exposing a silver plate to the vapour of iodine, by which it becomes covered with a stratum of iodide of silver which is sensitive to light, I stated to the Section that this fact had been known to me for some time, and that it formed the basis of one of the most curious of optical phænomena, which, as it did not appear to have been observed by M. Daguerre, I would describe to the Meeting. Place a small particle of iodine, the size of a pin's head, on a plate of silver, or on a piece of silver leaf spread on glass. Warm it very gently and you will shortly see the particle become surrounded with a number of coloured rings, whose tints resemble those of Newton's rings. Now, if these coloured rings are brought into the light a most singular phænomenon takes place, for the rings prove to be sensitive to the light, and their colours change, and after the lapse of a short time their original appearance is quite gone, and a new set of colours have arisen to occupy their places. These new colours are altogether unusual ones; they do not resemble anything in Newton's scale, but seem to conform to a system of their own. For instance, the two first colours are, *deep olive-green*, and *deep blue inclining to black*, which is quite unlike the commencement of Newton's scale. It will be understood that the outermost ring is here accounted the first, being due to the thinnest stratum of iodide of silver, furthest from the central particle. The number of rings visible is sometimes considerable. In the centre of all, the silver leaf becomes white and semitransparent like ivory. This white spot when heated turns yellow, again recovering its whiteness when cold; from which it is inferred to consist of iodide of silver in a perfect state. The coloured rings seem to consist of iodide of silver in various stages of development.

They have a further singular property, which is as follows: —It is well known that gold leaf is transparent, transmitting a bluish-green light, but no other metal has been ascribed as possessing *coloured transparency*. These rings of iodide of silver however possess it, being slightly transparent, and transmitting light of different colours. In order to see this, a small portion of the film should be isolated, which is best done by viewing it through a microscope.

I then related another experiment, in which a particle of iodine was caused to diffuse its vapour over a surface of mercury. In order to this, a copper-plate was spread over with

nitrate of mercury, and then rubbed very bright, and placed in a closed box along with a small cup containing iodine. The result was a formation of Newton's rings of the greatest splendour and of a large size. But they did not appear to be in any degree sensitive to light.

The next experiment related was as follows:—If a piece of silver leaf is exposed to the vapour of iodine, however uniform the tension of the vapour may be, yet it does not combine uniformly with the metal, but the combination *commences* at the *edge* of the leaf and spreads inwards, as is manifested by the formation of successive *bands of colour* parallel to the edge. Perhaps this is due to the powerful electrical effect which the sharp edges and points of bodies are known to possess, so that *electricity* may be either the *cause*, or the attending *consequence* of the combination of vapour with a metallic body.

Again:—if a minute particle of iodine is laid on a steel plate, it liquefies, forming an iodide of iron, and a dew spreads around the central point. Now, if this dew is examined in a good microscope, its globules are seen not to be arranged casually, but in straight lines along the edges of the minute striæ or scratches which the microscope detects even on polished surfaces. This is another proof how vapour is attracted by *sharp edges*, for the sides of those striæ are such.

The above extracts from the Athenæum will I think sufficiently show that I was acquainted with the effects of iodine vapour on silver surfaces at the time of the Birmingham Meeting, and consequently, prior to the publication of Daguerre's secret. For it will be in the recollection of men of science, that the publication of that important discovery was first received in England during the very week in which the meeting was held.

I am desirous to point out this circumstance, because it is connected with the early history of the Photographic art;—as I will explain.

Having in the year 1834 discovered the principles of Photography on *paper*, I some time afterwards made experiments on metal plates; and in the year 1838 I discovered the method of rendering a silver plate sensitive to light by exposing it to iodine vapours. I was at that time therefore treading in the steps of Daguerre, without knowing that he, or indeed that any other person, was pursuing, or had even commenced or thought of, the art which we now term Photography.

But as I was not aware of the power of mercurial vapour to bring out the latent impression, I found my plates of iodized silver deficient in sensibility, and therefore continued to use in preference my *photogenic drawing paper*. This was in 1838.

Some time after, viz. in August 1839, Daguerre published the account of his perfected process, which reaching us during the meeting of the British Association, gave rise to an animated discussion in *Section A*, and I took the opportunity to lay before the Section the facts which I had myself ascertained in *metallic photography*, and from the report which was given in the Athenæum of that communication I have taken the above extracts. On reading them over, I perceive a discrepancy in the result of my experiment on *mercury* exposed to iodine vapour from that given by Dr. Waller (p. 434), for which I cannot at present satisfactorily account.

London, 21st December, 1842. H. F. TALBOT.

XVI. *A further investigation of the Analytical Conditions of Rectilinear Fluid Motion. By the Rev.* J. CHALLIS, *M.A., Plumian Professor of Astronomy in the University of Cambridge**.

THE questions in the analytical theory of fluid motion, which I have recently discussed in various communications to this Journal, are of a fundamental character, and so long as any doubt remains as to the answers they should receive, the particular applications of the general theory will be involved in uncertainty. Trusting that this will be considered a sufficient apology for so often recurring to the subject, I proceed to inquire further respecting the analytical conditions of the rectilinear motion of fluids, being aware that my communication on this question to the December Number (S. 3. vol. xxi. p. 423.) stands in need of additional explanation.

It has been intimated to me that in the proof of rectilinear motion which I have deduced from the equation $u\,dx + v\,dy + w\,dz = \mathrm{V}\,dr$, I assume, without assigning any reason, that V is a function of a line r drawn always in the direction of the motion. It is true that when the equation is taken by itself, it does not necessarily follow that V is such a function because the left-hand side is integrable. The reason for the assumption is founded on the general equations of fluid motion, as I now proceed to show. I am ready to admit that the omission of the argument which follows is a defect in the former proof. The nature of the argument will be understood by referring first to the general equations of fluid *equilibrium*. If p be the pressure and ϱ the density at any point $x\,y\,z$, and X, Y, Z be the impressed forces in the directions of

* Communicated by the Author.

the axes of coordinates, those equations, it is well known, are,

$$\frac{dp}{dx} - \rho X = 0; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

$$\frac{dp}{dy} - \rho Y = 0; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

$$\frac{dp}{dz} - \rho Z = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

By multiplying (1.) by dx, (2.) by dy, and (3.) by dz, and adding, we obtain,

$$\left(\frac{dp}{dx} - \rho X\right) dx + \left(\frac{dp}{dy} - \rho Y\right) dy + \left(\frac{dp}{dz} - \rho Z\right) dz = 0. \quad (4.)$$

And if $dy = m\,dx$, $dz = n\,dx$, this equation becomes

$$\left(\frac{dp}{dx} - \rho X\right) + \left(\frac{dp}{dy} - \rho Y\right) m + \left(\frac{dp}{dz} - \rho Z\right) n = 0.$$

It is plain that by reason of the equations (1.), (2.), (3.), this last equation is satisfied whatever be m and n, and consequently that the increments dx, dy, dz are in no way related to each other. On this account, when the right-hand side of the equation

$$\frac{(dp)}{\rho} = X\,dx + Y\,dy + Z\,dz$$

is an exact differential, the integral may be taken from any one point of the fluid to any other. I am not aware that the specific reason for its being allowable to integrate between limits entirely arbitrary, has ever been in this manner referred to the equations (1.), (2.), and (3.).

Let us now turn to the hydrodynamical equations. For the sake of having as simple analytical expressions as possible, I shall confine myself to the equations of motion parallel to the plane of xy, these being sufficient for carrying on the argument. We shall thus have the two equations,

$$\frac{dp}{\rho\,dx} - X + \frac{du}{dt} + u\frac{du}{dx} + v\frac{du}{dy} = 0, \quad . \quad . \quad (5.)$$

$$\frac{dp}{\rho\,dy} - Y + \frac{dv}{dt} + u\frac{dv}{dx} + v\frac{dv}{dy} = 0. \quad . \quad . \quad (6.)$$

By multiplying (5.) by dx and (6.) by dy, and adding, an equation is obtained not generally integrable, but which becomes integrable by supposing $u\,dx + v\,dy$ to be an exact dif-

ferential. If $(d\,\phi) = u\,d\,x + v\,d\,y$, $(d\,\lambda) = \frac{(d\,p)}{\rho} - \mathrm{X}\,d\,x - \mathrm{Y}\,d\,y$, and $\mathrm{V}^2 = u^2 + v^2$, the result is,

$$(d\,\lambda) + \left(d\,.\frac{d\,\phi}{d\,t}\right) + \frac{1}{2}\,(d\,.\,\mathrm{V}^2) = 0, \quad . \quad . \quad . \quad (7.)$$

which is equivalent to

$$\left(\frac{d\,\lambda}{d\,x} + \frac{d^2\,\phi}{d\,x\,d\,t} + \mathrm{V}\,\frac{d\,\mathrm{V}}{d\,x}\right)d\,x + \left(\frac{d\,\lambda}{d\,y} + \frac{d^2\,\phi}{d\,y\,d\,t} + \mathrm{V}\,\frac{d\,\mathrm{V}}{d\,y}\right)d\,y = 0. \ (8.)$$

Now by reasoning like that applied to the equations of equilibrium, it appears from equation (8.) that when $u\,d\,x + v\,d\,y$ is an exact differential, the ratio of $d\,y$ to $d\,x$ cannot be of arbitrary value unless we have separately,

$$\frac{d\,\lambda}{d\,x} + \frac{d^2\,\phi}{d\,x\,d\,t} + \mathrm{V}\,.\,\frac{d\,\mathrm{V}}{d\,x} = 0, \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

$$\frac{d\,\lambda}{d\,y} + \frac{d^2\,\phi}{d\,y\,d\,t} + \mathrm{V}\,\frac{d\,\mathrm{V}}{d\,y} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (10.)$$

In this case only the equation (7.) may be integrated from any one point of the fluid to any other, and the arbitrary constant to be added is a function of the time only and not of coordinates. The integrals of equations (9.) and (10.) must evidently be identical with that of equation (7.), and therefore with each other. To satisfy this condition it is necessary that $\lambda + \frac{d\,\phi}{d\,t} + \frac{\mathrm{V}^2}{2}$ should be a function of a *single* variable, which is itself a function of x and y; and again, to satisfy this last condition, it is sufficient if ϕ be a function of that single variable, as will appear thus. Let $r_{\prime}$ be the variable. Then since $\phi = c$ is the equation of a surface of displacement, it follows that $r_{\prime} = c'$ is also its equation. Hence the surface of displacement is that of a cylinder having $r_{\prime}$ for radius. Consequently $r_{\prime}$, being always in the direction of the motion, is the same as r in the equation $u\,d\,x + v\,d\,y = \mathrm{V}\,d\,r$; and because $\mathrm{V} = \frac{d\,\phi}{d\,r}$, V, as well as ϕ, is a function of r. Also, as the motion is in the direction of the radii, and is the same at all points of the cylindrical surface, the effective accelerative force in passing from point to point of this surface is nothing. But if $d\,\sigma$ be the increment of a line drawn on the surface, the effective accelerative force $= \frac{d\,\lambda}{d\,\sigma}$. Hence $\frac{d\,\lambda}{d\,\sigma} = 0$, and λ is a func-

tion of r. Consequently equations (9.) and (10.) become

$$\left(\frac{d\lambda}{dr}+\frac{d^2\phi}{dr\,dt}+\mathrm{V}\frac{d\mathrm{V}}{dr}\right)\frac{dr}{dx}=0,$$

$$\left(\frac{d\lambda}{dr}+\frac{d^2\phi}{dr\,dt}+\mathrm{V}\frac{d\mathrm{V}}{dr}\right)\frac{dr}{dy}=0;$$

which, as the indeterminate factors $\frac{dr}{dx}$ and $\frac{dr}{dy}$ may be removed, are identical with each other and with equation (7.). This reasoning, which may be readily extended to motion in space of three dimensions, justifies the assumption in my last communication, that V is a function of r when $u\,dx+v\,dy+w\,dz$ is an exact differential which may be integrated from any one point of the fluid to any other; and the conclusion thence derived also holds good, viz. that in that case the motion is rectilinear.

The foregoing reasoning shows that the equations (9.) and (10.) are satisfied when ϕ is a function of a single variable r, such that $r^2=(x-a)^2+(y-b)^2+(z-c)^2$, a, b, and c being constant. To determine under what circumstances ϕ can be such a function, and the particular form of the function, recourse must be had to the equation of continuity. For incompressible fluids it is readily shown that ϕ may be a function of r whatever be the impressed forces, and that for motion in space of two dimensions $\frac{d\phi}{dr}$ varies inversely as r, and for motion in space of three dimensions, inversely as the square of r. But the equation of continuity for compressible fluids, when ϕ is assumed to be a function of r and t, is transformed into

$$0=\left(m^2-\frac{d\phi^2}{dr^2}\right)\frac{d^2\phi}{dr^2}-\frac{d^2\phi}{dt^2}-2\frac{d\phi}{dr}\cdot\frac{d^2\phi}{dr\,dt}$$
$$+\left(\frac{2m^2}{r}+\frac{\mathrm{X}(x-a)}{r}+\frac{\mathrm{Y}(y-b)}{r}+\frac{\mathrm{Z}(z-c)}{r}\right)\frac{d\phi}{dr},$$

which does not accord in giving ϕ a function of r and t unless the impressed force either vanishes or is a function of r.

It may be asked whether the supposition that ϕ is a function of a single variable, embraces *every* case in which $\lambda+\frac{d\phi}{dt}+\frac{\mathrm{V}^2}{2}$ is such a function. The following answer appears satisfactory. Since λ and ϕ are distinct variables, the

former relating to the arbitrary forces impressed, the latter to the arbitrary manner in which the fluid is put in motion, they must be *separately* functions of the single variable of which the whole quantity $\lambda + \frac{d\phi}{dt} + \frac{1}{2}\left(\frac{d\phi^2}{dx^2} + \frac{d\phi^2}{dy^2}\right)$ is a function. Consequently no supposition more general than that ϕ is such a function can be made. If, however, the motion be steady, so that $\frac{d\phi}{dt} = 0$, it is possible λ and V may be functions of a single variable while ϕ is not so. This can occur when the motion is parallel to a given plane in concentric circles about a fixed axis, and the velocity is a function of the distance (R) from the axis. Let $V = \phi(R)$, and $R^2 = x^2 + y^2$. Then $u\,dx + v\,dy = \phi(R)\left(\frac{y\,dx - x\,dy}{R}\right)$, which is not an exact differential unless $\phi(R) = \frac{C}{R}$. Also as the motion is by hypothesis uniform, the effective accelerative force in passing from point to point of a circle of motion vanishes; that is, $\frac{d\lambda}{d\sigma} = 0$. Hence λ is a function of R, and the equations (9.) and (10.) become for this instance identical with equation (7.). This is a very particular case of curvilinear motion, and appears to be the only exception to the theorem I am arguing for.

The most important result to which the preceding discussion leads is, that the arbitrary constant introduced by integrating equation (7.), cannot be considered a function of the time only and not of coordinates, merely because $u\,dx + v\,dy$ is an exact differential. That it may be so considered, the condition also of the independence of the variations dx and dy must be satisfied. Hence the general integral of the equation of continuity $\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} = 0$, which equation was formed by merely supposing $u\,dx + v\,dy$ to be an exact differential, will include cases in which the arbitrary constant above mentioned is a function of coordinates and not solely of the time. Let us take an instance. The general integral is

$$\phi = F(x + y\sqrt{-1}) + f(x - y\sqrt{-1}),$$

whence

$$u = F'(x + y\sqrt{-1}) + f'(x - y\sqrt{-1})$$

$$v = \sqrt{-1}\,\{F'(x + y\sqrt{-1}) - f'(x - y\sqrt{-1})\}.$$

Suppose $F'(x+y\sqrt{-1})=\frac{m}{2}(x+y\sqrt{-1})$, and $f'(x-y\sqrt{-1}) = \frac{m}{2}(x-y\sqrt{-1})$. Then $u = m\,x$, $v = -\,m\,y$, and $\frac{d\phi}{d\,t} = 0$. Taking y and x coordinates of a line of motion, $\frac{d\,y}{d\,x} = \frac{v}{u} = -\frac{y}{x}$. Hence $x\,y = c_{,}$ is the equation of the lines of motion, which are therefore hyperbolas. The equation giving the pressure becomes, when there is no impressed force,

$$p = C - m\,(x^2 + y^2).$$

If now C be a function of the time only, we shall have $p = 0$ at all points of the cylindrical surface of which the equation is $C = m\,(x^2 + y^2)$, and beyond this surface the fluid does not extend, because the value of p beyond this limit becomes negative. The boundary of the fluid is therefore at *all* times a cylindrical surface. But if this be the case at one instant, it cannot be so the next, since the velocity is the same at the same time at all points of the surface, but differs in direction. This contradiction arises from assuming C to be a function of the time only in a case of *curvilinear* motion for which $u\,d\,x + v\,d\,y$ is an exact differential.

It is of so much consequence to establish fully the conclusions I have now arrived at, that I shall be excused for adverting to the arguments of other writers which appear to militate against them. A paper by Mr. Stokes on the steady motion of incompressible fluids in the recently published part of the Transactions of the Cambridge Philosophical Society (vol. vii. part 3. p. 439.), contains a proposition, which, if it be correctly proved, is a direct contradiction of the views I have here maintained. The instance (in p. 439.) is that of the uniform motion of fluid, issuing from an orifice in a vessel containing an indefinite expanse of fluid. The equation applicable to this case, supposing the motion to be parallel to the plane of $x\,y$, is

$$p = Q - \tfrac{1}{2}(u^2 + v^2) + C, \quad . \quad . \quad . \quad . \quad (11.)$$

where $d\,Q = X\,d\,x + Y\,d\,y$. If the fluid were at rest we should have $p_{,} = Q + C_{,}$, and $C_{,}$ would be absolutely constant. In the case of motion C may be determined by considering the pressure and velocity at any point indefinitely distant, where p is indefinitely near to $p_{,}$ and u and v are indefinitely small. Hence C is indefinitely near to $C_{,}$. Mr. Stokes goes on to argue that C is therefore equal to $C_{,}$ and

absolutely constant; and differentiating on this principle equation (11.) with respect to x only, he obtains

$$\frac{dp}{dx} = \mathrm{X} - u\frac{du}{dx} - v\frac{dv}{dx}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (12.)$$

Also $\dfrac{dp}{dx} = \mathrm{X} - u\dfrac{du}{dx} - v\dfrac{du}{dy}$, by the usual equation.

Hence by subtraction, $v\left(\dfrac{du}{dy} - \dfrac{dv}{dx}\right) = 0.$

To this reasoning it may be objected, that when C is assumed to be absolutely the same as $\mathrm{C}_{\prime}$ there is no longer any motion; the fluid is at rest, and the last equation is satisfied, not because $\dfrac{du}{dy} = \dfrac{dv}{dx}$, but because u and v each $= 0$. Since a state of motion differs from a state of rest, C differs from $\mathrm{C}_{\prime}$ by an infinitesimal quantity of some order; and as this quantity, however small, is a function of the coordinates of a line of motion, it is not allowable to differentiate equation (11.) with respect to x only. The equation (12.), being obtained on a false principle, is not even approximately true. An example will illustrate this. Let the fluid descend by the force of gravity (g), between two planes perpendicular to the plane of $x\,y$, and inclined to each other at a given angle, and let the motion be parallel to the plane of $x\,y$. Suppose the lower surface of the fluid to be always bounded by a horizontal plane to which an arbitrary motion is given, and the upper free surface to be kept horizontal by the force of gravity. An instance of fluid motion similar to this I have considered in the Philosophical Magazine for January 1831, and in the Cambridge Philosophical Transactions (vol. v. part 2. p. 186.). By reasoning as in that instance, I find that the velocity is in straight lines directed to the intersection of the two planes, and in a given line varies inversely as the distance from the intersection. Also that the vertical velocity is the same at all points of the same horizontal plane. Hence, taking x and y vertical and horizontal distances from the intersection of the planes, if U be the vertical velocity, supposed uniform, at the vertical distance h, we shall have $u = \dfrac{\mathrm{U}h}{x}$, $v = \dfrac{\mathrm{U}hy}{x^2}$, and

$$p = \mathrm{C} - g\,x - \frac{\mathrm{U}^2 h^2}{2\,x^2}\left(1 + \frac{y^2}{x^2}\right), \quad . \quad . \quad . \quad . \quad . \quad (13.)$$

the integration being performed along a line of motion. If

now $\frac{y}{x} = \tan\theta$, and $x = \mathrm{H}$ at the upper surface of the fluid where $p = 0$, it will be found that

$$\mathrm{C} = g\,\mathrm{H} + \frac{\mathrm{U}^2 h^2}{2\,\mathrm{H}^2}\sec^2\theta.$$

Here then we have an instance in which $u\,d\,x + v\,d\,y$ is *not* an exact differential, whether H be finite or infinitely great, although in the latter case the part of C which contains the variable $\sec\theta$ is an infinitesimal of the second order. The differential of equation (13.) cannot in any case be taken except from point to point of a line of motion. This example and the reasoning which preceded it, may suffice to show that Mr. Stokes's argument is inadmissible.

In my last communication I gave reasons for concluding that $u\,d\,x + v\,d\,y + w\,d\,z$ is not necessarily a complete differential when the motion is small. Another argument leading to the same conclusion, I find in a note at p. 464 of vol. vii. part 3. of the Cambridge Philosophical Transactions. This point may now perhaps be looked upon as settled. But the author of that Note contends (at p. 462.) that $u\,d\,x + v\,d\,y + w\,d\,z$ is always an exact differential when the motion commences from rest. The following reasoning shows on the contrary that this proposition is also untenable. If $u = 0$, $v = 0$, and $w = 0$, when $t = 0$, each of these quantities contains some power of t as a factor, and we may therefore assume that $u\,d\,x + v\,d\,y + w\,d\,z = t^{\alpha}(\mathrm{U}\,d\,x + \mathrm{V}\,d\,y + \mathrm{W}\,d\,z)$, one at least of the quantities U, V, W not vanishing when $t = 0$. Now since t is unaffected by the sign of differentiation, if one of the quantities $u\,d\,x + v\,d\,y + w\,d\,z$, and $\mathrm{U}\,d\,x + \mathrm{V}\,d\,y + \mathrm{W}\,d\,z$ be an exact differential, the other must be also, whatever be the value of t. But the latter quantity is not necessarily an exact differential when $t = 0$; therefore $u\,d\,x + v\,d\,y + w\,d\,z$ is not necessarily an exact differential in the same case. If it be said that $\frac{d\,u}{d\,y}$ and $\frac{d\,v}{d\,x}$ each $= 0$, when $t = 0$, the answer is, the identity in analytical expression of these two differential coefficients is not proved unless the identity of $\frac{d\,\mathrm{U}}{d\,y}$ and $\frac{d\,\mathrm{V}}{d\,x}$ is proved, and the proof of *identity* is necessary before it can be concluded that the differential is exact.

Before quitting the subject I am desirous of adverting to the demonstration I gave in the Philosophical Magazine for last August (S. 3. vol. 21. p. 102.), which Mr. Stokes has objected to because it takes no account of the curvature of the lines of motion. Notwithstanding this omission, the proof is perfectly valid as far as it goes, and requires to make it complete only the enunciation of the following principle, which I suppose will be conceded, viz. that in fluid motion there is no *general* relation between the curvature of a surface of displacement and the curvature of a line of motion. This being admitted, when $u\,dx + v\,dy + w\,dz$ is the differential of a function of three independent variables, the equations

$$\frac{du}{dy} = \frac{dv}{dx}, \quad \frac{du}{dz} = \frac{dw}{dx}, \quad \frac{dv}{dz} = \frac{dw}{dy}$$

must be satisfied both when the curvature of the surface of displacement is considered apart from the curvature of the line of motion, and when the latter is considered apart from the former. The proof in the August Number takes account only of the curvature of the surface of displacement. Supposing α, β, γ to be the angles which the direction of the velocity V at the point $x\,y\,z$ makes with the axes of x, y, z respectively, it was shown that the equations to be satisfied (somewhat differently expressed), are,

$$\frac{du}{dy} - \frac{dv}{dx} = \frac{dV}{dy}\cos\alpha - \frac{dV}{dx}\cos\beta = 0, \quad . \quad . \quad \text{(I.)}$$

$$\frac{dv}{dz} - \frac{dw}{dy} = \frac{dV}{dz}\cos\beta - \frac{dV}{dy}\cos\gamma = 0, \quad . \quad . \quad \text{(II.)}$$

$$\frac{dw}{dx} - \frac{du}{dz} = \frac{dV}{dx}\cos\gamma - \frac{dV}{dz}\cos\alpha = 0; \quad . \quad . \quad \text{(III.)}$$

and that the only condition required for satisfying them is, that the surface of displacement be a surface of equal velocity.

Let now R be the radius of curvature of the line of motion at the point $x\,y\,z$, and let a, b, c be the angles which the plane of curvature makes with the axes of $x\,y\,z$ respectively. Then, abstracting from the curvature of the surfaces of displacement, that is, supposing these surfaces to be plane for a given small element of the fluid, it may without difficulty be shown that the equations to be satisfied are,

$$\frac{du}{dy} - \frac{dv}{dx} = \frac{V}{R} \cdot \left\{ \begin{array}{l} \cos\alpha\,\sqrt{\cos^2 b - \cos^2\beta} \\ \quad - \cos\beta\,\sqrt{\cos^2 a - \cos^2\alpha} \end{array} \right\} = 0, \quad \text{(IV.)}$$

$$\frac{d\,v}{d\,z} - \frac{d\,w}{d\,y} = \frac{\mathrm{V}}{\mathrm{R}} \cdot \left\{ \begin{array}{l} \cos\beta\sqrt{\cos^2 c - \cos^2\gamma} \\ \quad - \cos\gamma\sqrt{\cos^2 b - \cos^2\beta} \end{array} \right\} = 0, \qquad (\mathrm{V.})$$

$$\frac{d\,w}{d\,x} - \frac{d\,u}{d\,z} = \frac{\mathrm{V}}{\mathrm{R}} \cdot \left\{ \begin{array}{l} \cos\gamma\sqrt{\cos^2 a - \cos^2\alpha} \\ \quad - \cos\alpha\sqrt{\cos^2 c - \cos^2\gamma} \end{array} \right\} = 0. \qquad (\mathrm{VI.})$$

The general verification of these equations requires R to be infinitely great, that is, the motion to be rectilinear. Hence, when $u\,d\,x + v\,d\,y + w\,d\,z$ is an exact differential of a function of three independent variables, not only is the motion rectilinear, but the velocity must also be the same at all points of the same surface of displacement. This accords with what I have otherwise proved.

When the motion is parallel to the plane of $x\,y$, we have

$$a = 0, \quad b = 0, \quad c = \frac{\pi}{2}, \quad \alpha + \beta = \frac{\pi}{2}, \quad \gamma = \frac{\pi}{2},$$

and the complete expression for $\frac{d\,u}{d\,y} - \frac{d\,v}{d\,x}$ becomes

$$\frac{d\,\mathrm{V}}{d\,y}\cos\alpha - \frac{d\,\mathrm{V}}{d\,x}\sin\alpha + \frac{\mathrm{V}}{\mathrm{R}}\cos 2\,\alpha.$$

It is possible that this quantity may vanish when neither of the equations (I.) and (IV.) is satisfied. This will happen when the motion is in concentric circles about an axis, in which case we have

$$\mathrm{R}^2 = x^2 + y^2, \frac{d\,\mathrm{V}}{d\,x} = \frac{d\,\mathrm{V}}{d\,\mathrm{R}} \cdot \frac{x}{\mathrm{R}} = \frac{d\,\mathrm{V}}{d\,\mathrm{R}}\sin\alpha. \quad \text{So } \frac{d\mathrm{V}}{d\,y} = \frac{d\mathrm{V}}{d\,\mathrm{R}}\cos\alpha.$$

Hence $\left(\frac{d\,\mathrm{V}}{d\,\mathrm{R}} + \frac{\mathrm{V}}{\mathrm{R}}\right)\cos 2\,\alpha = 0.$

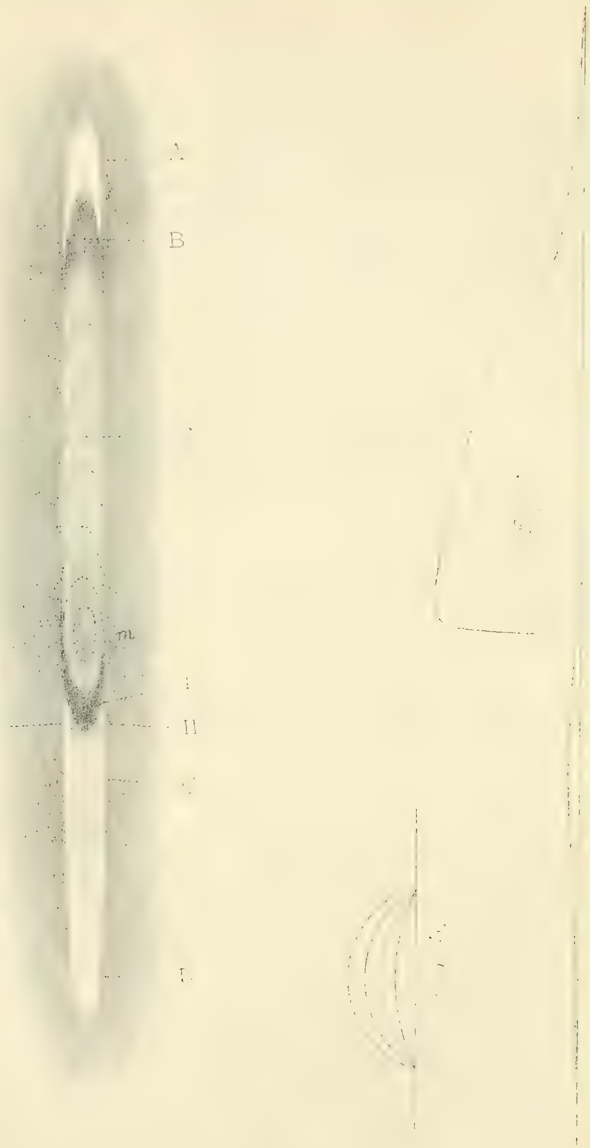
Therefore, $\frac{d\mathrm{V}}{d\mathrm{R}} + \frac{\mathrm{V}}{\mathrm{R}} = 0$, and by integration $\mathrm{V} = \frac{\phi\,(t)}{\mathrm{R}}$.

This is the singular case of curvilinear motion which I have already treated of in a different manner.

Cambridge Observatory, Dec. 17, 1842.

Postscript, Jan. 5, 1843.—The foregoing discussion supersedes the necessity of answering at any great length Mr. Stokes's reply in the January Number. All that is urged by Mr. Stokes in p. 55, is sufficiently answered by the reasoning which has conducted me to the equations (9.) and (10.), and by the consequences resulting from these equations. The principle on which the independence of the variations $d\,x$, $d\,y$,

Fig. 1.



$d z$ is established, is, I think, essential to complete the general theory of hydrodynamics, and deserves the particular attention of mathematicians engaged on this subject.

The kind of motion spoken of by Mr. Stokes at the top of p. 56, is sufficiently defined by its being *common* as to magnitude and direction to all the parts of the fluid. Such motion, I said, must be *considered* to be eliminated from the hydrodynamical equations. For if it were included in them, the terms relating to it would separate themselves from the terms relating to fluid motion, being in fact the same as if the fluid were supposed to be solid.

I understand Mr. Stokes to argue in the concluding paragraph of his reply, that if $u\,d\,x + v\,d\,y + w\,d\,z$ be an exact differential at one instant, when powers of the velocity above the first are omitted, it will be so always, the smallness of the velocity being the reason for omitting the higher powers, but not the reason that the differential is exact. If this is what is meant I quite agree with it. It is a very different thing to argue that $u\,d\,x + v\,d\,y + w\,d\,z$ is an exact differential *because* the motion is small. I have already pointed out the fallacy of this argument.

XVII. *On the Action of the Rays of the Solar Spectrum on Vegetable Colours, and on some new Photographic Processes.* By Sir John F. W. Herschel, *Bart., K.H., F.R.S.*

[Continued from p. 21.]

Turmeric.—*Further proofs of the continuation of the visible Prismatic Spectrum beyond the extreme Violet.*

185. The action of light on paper coloured with the alcoholic tincture of turmeric is but feeble. If long continued, however, it is whitened in the region of the blue and violet rays, from + 10 to + 43, or thereabouts, the maximum being at + 23·5. The paper browned by carbonate of soda is somewhat more sensitive, especially when wet, in which case an abruptly terminated action is perceptible in the red region, giving rise to a double maximum at −10·0 and +22·5, with an intermediate minimum at −4·0. I should not have thought it necessary, however, to mention this paper, but on account of a remarkable peculiarity in its reflective power, in virtue of which it renders very plainly visible a prolongation of the spectrum beyond the extreme violet, in the region of what I have termed in my last paper, the Lavender rays. As the experiment is easily made, and affords a ready method of rendering visible this part of the spectrum, I shall describe, with

some minuteness, the appearances which presented themselves in my experiments, and which seem to place the real existence of those heretofore undescribed luminous rays beyond all reasonable objection, should any doubt have arisen as to the interpretation of the phænomenon described in my former paper (Art. 59.).

186. Paper stained with tincture of turmeric is of a brilliant yellow colour, and in consequence, the spectrum thrown on it, if exposed in the open daylight, is considerably affected in its apparent colours, the blue portion appearing violet, and the violet very pale and faint; but beyond the region occupied by the violet rays is distinctly to be seen a faint prolongation of the spectrum, terminated laterally, like the rest of it, by strait and sharp outlines, and which in this case affects the eye with the sensation of a pale yellow colour. Comparative measures were carefully taken of the spectrum so prolonged and of the ordinary spectrum as seen projected on white paper, the results being as follows (see Plate I. fig. 6.):—

	Part.
Length of the spectrum Y L from the fiducial point Y to the visible termination L, as seen (with the naked eye) on the turmeric paper; corrected for $\odot$'s semidiameter	= 56·6
Length Y V from the same fiducial point to the visible termination, as similarly seen when projected on white paper	= 40·4
Prolongation rendered visible by projection of the spectrum on turmeric paper......................	= 16·2

187. The day on which this experiment was first made (May 27, 1841) was serene and clear, but being aware that in certain states of the atmosphere a vertical beam of halo-light passes through the sun, which in a meridional position of that luminary *might* give rise to a perceptible prolongation both upwards and downwards (though in fact no such prolongation was perceived at the red end), it was often repeated, and always with the same result, on subsequent occasions, whether the sun were on or near the meridian, or otherwise. Comparative trials, also with other yellow papers, fully satisfied me of the cause being traceable to a peculiarity in the colouring material, as to its reflective powers. In particular, a certain paper (No. 1055.) coloured with the juice of *Chryseïs californica*, whose tint was almost identical with that of the turmeric paper, only somewhat *brighter*, was tried, and the spectrum measured on this paper was found to terminate precisely at 44·0, i. e. (correcting for semidiameter) at 40·4, the very same as if white paper had been used.

188. To test the matter yet more pointedly, a strip of tur-

meric paper was fixed on the *Chryseïs* paper, so that its edge should bisect the spectrum longitudinally from end to end, the preceding half of the sun's lengthened image being received on the one paper, and the following half on the other. The papers thus arranged were so similar as hardly to be distinguished when simply laid in sunshine, but when illuminated by the spectrum, as above described, the half of it on the turmeric side was plainly seen to extend far beyond the other, as represented in fig. 6.

189. Hitherto I have met with only one other coloured paper which possesses a similar character in respect of its reflective power, and that by no means in so high a degree. To prepare it, the alcoholic tincture of the dark purple dahlia must be alkalized by carbonate of soda. The mixture is vivid green, which is also, at first, the colour of paper stained with it. But this colour changes in about twenty-four hours to a fine yellow, a little inclining to orange, after which it is remarkably permanent, and very little sensible to photographic impression. On this, as on the turmeric paper, the prolongation of the spectrum appears as a pale yellow streak. And if such, rather than lavender or dove-colour, should be the true colorific character of these rays, we might almost be led to believe (from the evident reappearance of redness mingled with blue in the violet rays) in a repetition of the primary tints in their order, beyond the Newtonian spectrum, and that if by any concentration rays still further advanced in the "chemical" spectrum could be made to affect the eye with a sense of light and colour, that colour would be green, blue, &c., according to the augmented refrangibility.

190. *Cases of negative Photographic Action on Vegetable Tints.*—Among a collection of plants which I made at the Cape of Good Hope, and have succeeded in rearing in England, occurred three species of a genus allied to Anthericum, with brilliant yellow flowers in lengthened spikes, and highly characteristic furred anthers, to which I am not botanist enough to assert the correct application of the name Bulbine, assigned to them by a friend in Cape Town. Of these three species, two (*Bulbine bisulcata* and. . . . .) yield from the green epidermis of their leaves and flower-stalks a bright yellow juice which darkens rapidly on exposure to light, changing at the same time to a ruddy brown. Exposed to the spectrum, the less refrangible rays are found inoperative, either in inducing the change of tint, or in preserving that portion of the paper on which they fall from the influence of dispersed light. The negative action commences at the fiducial yellow, is very feeble as far as + 10, where it begins to

increase, and is strong at + 23, where the maximum of effect is situated. Hence it degrades more slowly, is still pretty strong at + 60, and may be traced as far as 80, being therefore nearly commensurate with the spectrum impressed on nitro-argentine paper, a range of action unique, so far as my experience goes in vegetable photography. The species experimented on is that which (supposing it undescribed) I should be disposed to call *triangularis*, from the angular section of its long, slender, smooth, solid leaves; which, with the singular character of its juice, may serve to identify the species, my own specimen (a single one) having been destroyed by insects after flowering superbly. The ultimate tint acquired by the juice is a deep brown, to which it also passes in darkness, but much more slowly. The juices of both species, however, have the same photographic characters.

191. *Cheiranthus cheiri, Wall-flower.*—A cultivated double variety of this flower, remarkable for the purity of its bright yellow tint, and the abundance and duration of its flowers, yields a juice when expressed with alcohol, from which subsides, on standing, a bright yellow, uniform, finely divided fæcula, leaving a greenish yellow transparent liquid, only slightly coloured, supernatant. The fæcula spreads well on paper, and is very sensitive to the action of light, but appears at the same time to undergo a sort of chromatic analysis, and to comport itself as if composed of two very distinct colouring principles, very differently affected. The one on which the intensity and sub-orange tint of the colour depends is speedily destroyed, but the paper is not thereby fully whitened. A paler yellow remains as a residual tint, and this, on continued exposure to light, so far from diminishing in tone, slowly darkens to brown. Exposed to the spectrum, the paper is first speedily reduced nearly to whiteness in the region of the blue and violet rays. More slowly, an insulated solar image is whitened at − 10·5, or in the less refrangible portion of the red, and the impressed spectrum assumes the type represented in fig. 7, where m Y = − 10·5; m' Y = + 13·0; Y c = + 55. The exposure continuing, a brown impression begins to be perceived in the midst of the white streak, which darkens very slowly from + 18·6 to + 42. It never attains any great intensity, but presents a singular appearance in the midst of the white train previously eaten out.

192. The juice in question contains gallic acid, and probably tannin, as is evident from its striking a strong black with persalts of iron. The gallic acid itself (whose singular properties, in conjunction with nitrate of silver, have been developed by Mr. Talbot, as the basis of his all but magical

process of the calotype*) is affected also negatively by light. Paper washed with its spirituous solution and partially covered, being exposed several months in a window, was found pretty strongly darkened in all the exposed portion. The action is too slow for prismatic analysis, and I am far from attributing to the presence of this acid the phænomenon above recorded. It would rather appear as if some portion of a more decidedly negative ingredient analogous to that which exists in the *Bulbine*, were present. As regards the positive ingredient, I may mention here the common Marigold (in which also the colour resides in an insoluble fæcula) as a flower in which the colouring principle is probably identical both with this and with that of the *Corchorus Japonica*, since it comports itself in the very same manner under the spectrum,—is nearly, or quite as sensitive, and is moreover fugitive, even when carefully defended from light, giving photographs which cannot be preserved. Many other flowers also contain in their juices a portion of this identical, or a very similar yellow principle, probably in a state of greater solubility, and thence disposed to the absorption of oxygen. Thus the juice of a fine purely yellow species of *Mimulus*†, if expressed, with or without alcohol, though vividly yellow in the first moments of expression, passes almost instantly to dirty green, and loses its sensibility to light; but if crushed on paper and immediately dried, the petals give a bright yellow stain which agrees in sensibility, and in the type of the impressed spectrum with the *Corchorus*. The *Ferranea undulata*, a dark brown flower, yields, when expressed, a dull green juice, which, spread on paper and dried, turns very speedily blue under the influence of the blue and violet rays of the spectrum; owing to the destruction of this yellow principle, which, mingling with the substratum of blue (itself a much more indestructible tint), gives it its natural tinge of green. A similar destruction, of probably again the same yellow matter, in the colour of the American Marigold‡, causes its tint to pass rapidly in sunshine from brown to green, after which continued exposure produces no further change. The yellow colour of fresh bees'-wax and of palm-oil, are also, I doubt not, referable to the same, or a nearly similar

* Preparations of the gallic acid in conjunction with silver, are noticed by me in my former paper as forming a "problematic exception" to my general want of success in procuring at the very outset of my photographic experiments (in February 1839), papers more sensitive than the simple nitrated or carbonated ones. The problematic feature consisted in spontaneous darkening of the papers laid by to dry in the dark, so at least then considered, but really arising doubtless from light incident on them in their preparation. Acetate of silver was used in their preparation.

† *Mimulus Smithii* (Lindl.). ‡ French Marigold, *Tagetes Patula*.

colouring matter, both being very speedily bleached by exposure to light.

193. *Viola odorata.*—Chemists are familiar with the colour of this flower as a test of acids and alkalies, for which, however, it seems by no means better adapted than many others; less so, indeed, than that of the *Viola tricolor*, the common purple Iris, and many others which might be named. It offers, in fact, another, and rather a striking instance of the simultaneous existence of two colouring ingredients in the same flower, comporting themselves differently, not only in regard to light but to chemical agents. Extracted with alcohol, the juice of the violet is of a rich blue colour, which it imparts in high perfection to paper. Exposed to sunshine, a portion of this colour gives way pretty readily, but a residual blue, rather inclining to greenish, resists obstinately, and requires a very much longer exposure (for whole weeks indeed) for its destruction, which is not even then complete. Photographic impressions, therefore, taken on this paper, though very pretty, are exceedingly tedious in their preparation, if we would have the lights sharply made out.

194. The residual tint thus outstanding, after long exposure, is turned, not green, but yellow, by alkalies; or, if greenish at first, a very few hours suffice for the destruction of the slight remnant of blue, and the consequent appearance of the yellow colour. Reasoning on this fact, as well as on the action of light above mentioned, it seems highly probable that the tincture in question holds in solution two distinct colouring principles, of which the one (greatly preponderant in quantity) is destructible by light, and either destroyed or turned green by alkalies; the other, indestructible by light, and either naturally yellow in colour or changeable into yellow by alkaline agency.

195. This view of the composite nature of the colour in question receives corroboration from the habitudes of the alcoholic tincture above mentioned, when rendered green by admixture of carbonate of soda. On making this addition it becomes evident that a large amount of colour has been destroyed; the green tint imparted by it to paper being far less intense than might be expected from the intensity of the original hue, and from the trifling dilution caused by the small quantity of alkaline liquid required to effect the change. What remains is a fine green; but when exposed to light, the blue constituent alone of that green is destroyed, and a residual tint of pure yellow, which is very indestructible by light, is left. Exposure of a slip of such paper to the spectrum proves this change to be operated almost wholly by rays less

refrangible than the fiducial yellow. A slight discoloration is perceived in the indigo-blue rays (at about + 30), but the *green* appears quite inactive.

196. In the case of the purple Iris mentioned above, when turned green by the same reagent, the tint is fuller and richer, as well as, photographically, more sensitive, and the residual yellow less abundant. And in this case the resistance of the tint to rays of its own colour is very strongly marked. The spectral impression consists, in fact, of two portions clearly separated by the whole of the interval occupied by the green and greenish blue rays, conformably to the general remark in Art. 170.

197. *Sparaxis tricolor?, var.—Stimulating effect of alkalies.*—Among a great many hybrid varieties of this genus, lately forwarded to me from the Cape, occurred one of a very intense purplish brown colour, nearly black. The alcoholic extract of this flower in its liquid state is rich crimson brown. Spread on paper it imparted a dark olive green colour, which proved perfectly insensible to very prolonged action, either of sunshine or the spectrum. The addition of carbonate of soda changed the colour of this tincture to a good green, slightly inclining to olive, and which imparted the same tint to paper. In this state, to my surprise, it manifested rather a high degree of photographic sensibility, and gave very pretty pictures with a day or two of exposure to sunshine. When prepared with the fresh juice there is hardly any residual tint, but if the paper be kept, a great amount of indestructible yellow remains outstanding. The action is confined chiefly to the negative end of the spectrum, the maximum being at − 8·0, and the sensible limits of the impression (corrected for semidiameter) being − 11·0 and + 56·4, of which, however, all but the first five or six parts beyond the fiducial yellow show little more than a trace of action. A photograph impressed on this paper is reddened by muriatic acid fumes. If then transferred to an atmosphere of ammonia, and when supersaturated the excess of alkali allowed to exhale, it is fixed, and of a dark green colour. Both the tint and sharpness of the picture, however, suffer in this process.

198. *Red Poppy—Papaver Rheum?*—Among the vegetable colours totally destroyed by light, or which leave no residual tint, at least when fresh prepared, perhaps the two most rich and beautiful are those of the red poppy, and the double purple groundsel (*Senecio splendens*). The former owes its red colour in all probability to free carbonic acid, or some other (as the acetic) completely expelled by drying, for the

colour its tincture imparts to paper, instead of red is a fine blue, very slightly verging on slate-blue. But it has by no means the ordinary chemical characters of blue vegetable colours. Carbonate of soda, for instance, does not in the least degree turn the expressed juice green; and when washed with the mixture, a paper results of a light slate-gray, hardly at all inclining to green. The blue tincture is considerably sensitive, and from the richness of its tone and the absence of residual tint, paper stained with it affords photographic impressions of great beauty and sharpness, some of which will be found among the collection submitted with this paper for inspection.

199. *Senecio splendens.*—This flower yields a rich purple juice in great abundance and of surprising intensity. Nothing can exceed the rich and velvety tint of paper tinted with it while fresh. It is, however, exceedingly insensible to light, and it is only by an exposure continued for many weeks, that it is possible to get a complete photographic impression of a picture on it. Still, when obtained, owing to the whiteness of the ground, the effect is pleasing, and would be beautiful were it not that the general tint suffers somewhat in its tone and softness of surface.

200. The juices of the leaves, stalks, roots, &c. of plants afford a wide and interesting field of photographic inquiry. Those of leaves are for the most part green, and being usually loaded with gum, extractive, &c., are difficult of manipulation. Such as I have tried, which spread well on paper, as the elder, the potatoe, the night-shade, and a few others, proved very sensitive if gathered when just in the perfection of their development, and in full vitality. As the season advances they lose much of their sensibility. There is much uniformity in the action of the spectrum on their colour, in consequence of which I shall content myself with describing the phænomena as exhibited on that of the elder leaf. The type of the impressed spectrum in this case is, as in fig. 8, exhibiting a strong decided maximum of action, giving rise to a nearly insulated solar image at $-$ 11·5, or almost at the extremity of the red rays. The colour of this image was a pale yellowish pink or flesh colour; from thence the action is feeble, with two subordinate minima (at $-$ 5·0, $+$ 6·8), with a slight intermediate maximum at 0·0, and beyond these (or about the termination of the green) the action again increases; reaches another maximum at $+$ 20·0, after which it declines gradually, and beyond $+$ 45 ceases to be traceable. Photographic pictures may be taken readily on such papers, half an hour in

good sun sufficing; but the glairy nature of the juices prevents their being evenly tinted, and spoils their beauty*.

201. The ruddy tint which comes out when the green is destroyed by light, is in all probability that which gives the whole colour to sere and withered leaves, whether simply disclosed by the destruction of the green which masked it in the live state of the leaf, or matured by exposure to light during the whole season, either out of the elements of the green colouring matter destroyed, or from the other juices of the vegetable. It deserves to be noticed in connexion with this, that all the lively vegetable greens have a large portion of red in their composition, and are in fact dichromatic. A good example of such a colour is a solution of sap-green, which, used as a prism, is seen to transmit both red and green rays, separating them by a broad interval which increases as the thickness or density of the solution is increased; the red ultimately preponderating, and the green being extinguished. If we view a garden or shrubbery through a glass of a pure and deep red colour, every shrub, such as the laurel, of a lively and brilliant foliage, and especially green grass, will appear scarlet. Under such circumstances, a grass-plot, seen in contrast with a gravelled walk, shows as light on darkness, contrary to their habitual order of illumination. So great is the quantity of extreme red light reflected by a green sward, as actually to appear bright in opposition to clear blue sky seen through the same glass in the quarter of the heavens opposed to the sun, and that at noon day. The aspect of nature, indeed, when viewed through coloured glasses, is fraught with curious and interesting matter of optical remark; but to give them their full effect they must not be merely applied to one eye for a few moments, as in the use of Claude Lorraine glasses. They should be worn as spectacles, both eyes being used, all lateral light carefully excluded by black velvet fringes, and their use continued till the pupil is fully dilated and the eye familiarized with the intensity and tone of the illumination. So used, not only are the ordinary relations of all lights and colours strangely and amusingly deranged, but contrasts arise between colours naturally the most resembling, and resemblances between those naturally the most opposed. We become aware of elements in the composition of tints we should otherwise never have suspected, and the singularities of idio-

* I have not operated on chlorophyle (the green colouring matter of leaves) in a state of purity, owing to the nicety required in its preparation.

chromic vision, which seem so puzzling when related, cease to be matter of any surprise *.

[To be continued.]

XVIII. *Supplementary Remarks relative to certain Arguments against the Theory of Molecular Action according to* Newton's *Law. By the Rev.* P. Kelland, *M.A., F.R.SS.L.& E., F.C.P.S.,&c., Professor of Mathematics in the University of Edinburgh, late Fellow and Tutor of Queen's College, Cambridge.*

BEING called upon by Mr. Earnshaw in the Number of the Philosophical Magazine for December (S. 3. vol. xxi, p. 444.) to reply directly to a series of categorical questions, and being at the same time desirous of keeping the discussion as simple and disentangled as possible, I trust I may be allowed the benefit of an early insertion of the following remarks, as proper to precede anything which either party may add to the controversy. I believe we are all agreed in desiring that this point should be cleared up as a preliminary step. I will therefore, for the present, confine myself exclusively to it. The matter in question resolves itself into the answers to the following queries.

1. Does it follow *from the hypothesis* which I have adopted that the three following expressions are equal,—

$$2\,\Sigma\left(\phi\, r + \frac{\mathrm{F}\, r}{r}\,\delta\, x^2\right)\sin^2\frac{k\,\delta\,\varrho}{2},$$

$$2\,\Sigma\left(\phi\, r + \frac{\mathrm{F}\, r}{r}\,\delta\, y^2\right)\sin^2\frac{k\,\delta\,\varrho}{2},$$

$$2\,\Sigma\left(\phi\, r + \frac{\mathrm{F}\, r}{r}\,\delta\, z^2\right)\sin^2\frac{k\,\delta\,\varrho}{2},$$

they being the proportions of force to disturbance, parallel respectively to x, y, and z? On the assumption of their equality depend the arguments adduced at pp. 341 and 343 of the Philosophical Magazine for November, by my respective opponents.

* The late celebrated optician Mr. Troughton, who was a remarkable instance of this sort of vision, informed me that he could not distinguish the scarlet coats of a regiment of soldiers from the green turf on which they were drawn up, nor ripe cherries from the leaves of the tree which bore them. His eyes, however, were perfectly sensible to rays of every refrangibility *as light*, but the spectrum afforded him only the sensations of two *colours*, which he termed blue and yellow; pure red and pure yellow rays exciting in his mind the same sensation. [See a paper by the late Mr. G. Harvey *on an anomalous case of vision*, reprinted from the Transactions of the Royal Society of Edinburgh, in Phil. Mag., S. 1. vol. lxviii, p. 205.—Edit.]

2. Does it follow *from the arguments* I have used that they ought to be equal?

3. Do I make an axis of coordinates to coincide with the direction of transmission?

1. I say it does not follow from any *hypothesis* that these quantities should be equal; nor even that the equations should assume the form assigned to them at p. 162, unless one of the axes of coordinates be that of transmission. This I suppose my opponents will admit, at least Mr. O'Brien himself points out the fact, when he says (Phil. Mag. for June, 1842, p. 485), "I assert that the equations at the foot of p. 162 of the Transactions of the Camb. Phil. Soc. are *essentially erroneous*; they ought to have been in the form," &c. He explains himself afterwards, (Phil. Mag. for November, p. 345, P.S.) by making me exclude the case (the only one I did *not* exclude) in which an axis of coordinates coincides with the direction of transmission. Mr. O'Brien then admits (what is quite correct) that even the *form* of the equations, to say nothing of the equality of the quantities themselves, is not a necessary consequence of the hypothesis of molecular forces. I may add that the true form corresponding with the case supposed by Mr. O'Brien, will be found in the Transactions of the Camb. Phil. Soc. p. 331. Any arguments, then, levelled at the Newtonian Law through the supposed equality of these quantities, fall to the ground.

2. But does it follow *from the arguments* I have used that they ought to be equal? If it does, no consequences will follow, except the obvious one, that I have committed an error in *one* proposition. But this clearly does not follow from my arguments. The quantities are doubtless denoted by the same letter n; and Cauchy designates them by the same letter s, almost always. At p. 139 of the *Exercices d'Analyse*, we find $s^2 = \mathrm{E}$, $s^2 = \mathrm{E} + \mathrm{F}\,k^2$, the one directly under the other. But it may be urged that I have myself apparently sanctioned the supposition that the quantities are equal, by what I say at p. 163. I have already twice over admitted that some little confusion appears in this place. But if this be insufficient, I think the right way to determine whether I did consider the quantities to be equal or not, is to turn to where I have applied them. It will be found that, far from supposing this to be the case, I have *proved* them to be *unequal*. Let me point out where. In the Trans. of the Camb. Phil. Soc. pp. 179–180, and p. 268, for v differs from n only by a constant multiplier: in the Philosophical Magazine for May, 1837, p. 340, and in my Theory of Heat, p. 155. I assert, moreover, that I have never treated the three quantities as equal—if I have, let my opponents say where.

3. Do I not make an axis of coordinates to coincide with the direction of transmission? On this depends the correctness of my equations. Their very *form* is determined by the fact that I do. It has been shown above that they assume an essentially different form in the contrary case.

At p. 161, I say "...we might at once suppose the direction of transmission to be the axis of y, and put δy for $\delta \varrho$; this, however, I shall not do, as it does not appear necessary, and it is convenient to retain the symbol ρ, on other accounts to be noticed hereafter. The above remarks will be mainly useful in pointing out to us what are the quantities to be rejected in our equations of motion." I refer to this when I say (p. 162) "bearing in mind the remark above made with respect to ρ." Now I am quite sensible this might have been better and more clearly stated. But the reason why I did not put the symbol y for ρ, or rather why I changed y into ϱ, as I actually did, was this: I wished to retain a symbol which might be converted into x, y, or z at pleasure; and my readers will find I made ϱ to represent in succession x and z at p. 166, and y at p. 179. I will add further, that I never changed it into *anything else*. I never supposed it to incline to the axes. But the most satisfactory way of ascertaining how I *understood* my equations, will be to inquire how I interpreted and used them. I assert—*always* as having an axis of coordinates along the axis of transmission. Let my opponents point out one place in all my writings where I have done otherwise. It would be tedious for me to go over all I have written, and extract every passage where the form occurs. It will suffice that I point out a few. Trans. Camb. Phil. Soc. vol. vi. pp. 179, 239, 245, &c.; Phil. Mag. for May, 1837, p. 336; Theory of Heat, p. 146. In all these places, and in every other in which these equations occur, the direction of transmission coincides with one of the axes of coordinates.

I proceed, now, in conclusion, to answer Mr. Earnshaw's questions at p. 444 of the Magazine for this month (Dec.), or rather to answer one of them, so as to render the rest nugatory.

1. "Does Professor Kelland admit that I have satisfactorily proved that the quantity n used in his memoir on dispersion, is equal to zero?"

I answer NO. And wherefore? Because the proof rests on the assumption that three quantities are equal, which are essentially and necessarily *not so*. One of them *must* be different from the others. If any one thinks otherwise, let him prove that they are equal, or let him point out an error in my proof of their inequality.

Edinburgh, December 6, 1842.

P.S. Should any of my readers be desirous of seeing a proof of the inequality of the three expressions, elsewhere than in my own writings, they will find it at p. 301 of the *Exercices d'Analyse* of M. Cauchy. In a paper, part of which is already in the hands of the Editor, I have proved that my equations give rise, by the most simple process, to M. Cauchy's results. This is a sufficient guarantee for their correctness, and a strong illustration of the importance of the method which I have adopted.

P.S. Jan. 4, 1843.—The Philosophical Magazine for this month, with the united reply of Mr. Earnshaw and Mr. O'Brien, has just reached me. The object of the arguments in that paper is to show that what I termed a misprint or mistranscription, is in reality a *mistake*. Now suppose I grant them this for the sake of getting along, what do they gain by it? Do I thereby "confess that my fundamental equations are essentially erroneous" (O'Brien, p. 22)? No such thing. I confess no more than this—that there is an error in the equations, which error has *never* been propagated to other parts of my writings, and which has been corrected by myself in the Philosophical Magazine for May 1837. If then these gentlemen choose to *call* it a mistake, let them do so, and we will proceed to our arguments. But let not my readers be deceived by assertions. If this *is* an error in my *fundamental* equations, I *again* demand, where are these erroneous equations used by me? The case then stands between us as before. To follow their three positions:—1. I have never admitted the three n's as equal, although in the place in question they were denoted by the same letter. 2. I have never applied the equations without the limitation (relative to the direction of transmission) which they deem necessary. And 3. I have taken, and do still take, the blame of leading these gentlemen astray to myself.

In their P.S. is exhibited what is more tangible, an argument against the theory, and another against the consequences which I drew. They are,—1, that because $v^2 + v'^2 + v''^2 = 0$, $v = 0, v' = 0, v'' = 0$. Every one is aware that there are two ways of satisfying these equations, viz. (1) by the method which they give, and (2) by supposing that one (or two) of the quantities is impossible. Now the first cannot be true, for the nature of the function $\Sigma\, m\left(\frac{1}{r^3} - \frac{3\,\delta\, x^2}{r^5}\right) \sin^2 \frac{\pi\,\delta\, y}{\lambda}$ is such that in some cases it *must* depend on the relation of $\delta\, y$ to λ.

And further, M. Cauchy, in the place cited above, has determined the value of this function, and based an important

argument on it against the Newtonian Law (*Exercices d'Analyse*, p. 304). But that the second solution is the true one, will appear from considering that it only changes a circular function into an exponential one, which will, in general, not appear in the operations from its *coefficient* being zero.

2. But we have an argument against the solution, or rather an assertion, that the appearance of an exponential in place of a circular function "*violates the hypotheses* on which the reductions and transformations in the former part" (I should like to know what part) "of my paper are effected." If they mean those in p. 158, and which they refer to, I reply, so does *Newton's* problem of central forces violate his hypothesis of motion in an ellipse about the centre. But if these gentlemen assert that exponentials cannot be used for circular functions, and *vice versâ*, I refer them to the memoir of Cauchy just quoted, where nothing but exponentials are used, and to my 'Theory of Heat,' p. 156. I may add that this objection is so vaguely and cautiously stated, that I do not imagine the writers seriously entertain any belief in its force, but rather throw it out as a probable difficulty.

XIX. *On the Action of the Rays of the Solar Spectrum on the Daguerreotype Plate.* *By Sir* J. F. W. Herschel, *Bart., K.H., F.R.S., &c.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

1. PROFESSOR Draper of New York having, in his communication to the Philosophical Magazine for November last (Art. LXII.), referred to a specimen of a Daguerreotyped impression of the solar spectrum obtained by him in the south of Virginia as having been forwarded by him to me through your obliging intervention, I should hardly be doing justice, either to his urbanity or to the beauty of the specimen itself as a joint work of nature and art, were I to forbear acknowledging its arrival and offering a few remarks on it. And I do so the more readily, because, though forced to differ with him in some of the conclusions he has drawn from it, I recognize in him a zealous and effective contributor to this most interesting branch of scientific inquiry, and the only one, so far as I am aware, besides myself who has attacked it in the only mode in which it can lead to distinct and definite results, that of prismatic analysis.* It can never be too often repeated, that the use of coloured glasses

* Since this was written, M. Becquerel's interesting paper on the Spectrum, read to the French Academy, June 13, 1842, has come into my hands. M. Becquerel has also used the prism, and with excellent effect,

in such inquiries, as a substitute for such analysis, in the present state of our knowledge of the absorptive powers of such glasses, serves only to confuse and mislead. In illustration of this proposition I need only refer to the statements of Professor Moser* as to the action of the green and yellow rays on the iodide of silver, in his papers on the process of vision and on invisible light; statements which, embodying, at least in intention, the results of elaborate and no doubt most carefully conducted experiments, are rendered perfectly unintelligible to one who has studied the subject with the aid of the prism, by his continual (and as it would almost appear systematic) substitution of the apparent (or absorptive) colours of glasses for the *prismatic* colours of rays, which he appears to assume that such glasses insulate in a state at least approaching to purity; an assumption unwarranted by the whole tenor of the phænomena of absorption, and in the case of *yellow* glasses most particularly open to objection, as any one may readily satisfy himself by looking through such a glass at a prismatic spectrum, and by referring to Art. 103. of my paper "On the Chemical Action of the Rays of the Solar Spectrum," &c. (Phil. Trans., 1840), and repeating the experiments there described.

2. Professor Draper's specimen consists of a Daguerreotype silver plate, about $3\frac{1}{2}$ inches by 3 inches, on which is exhibited the impress of the spectrum in the form of a streak 3·3 inches, or thereabouts in length, and 0·08 inch in breadth, the edges being perfectly rectilinear and sharply defined throughout the whole length until within about a third of an inch from either termination, where they curve into an elliptic form so as to terminate the impression with two very elongated semiellipses, which are also very faintly and feebly marked. This, together with the very high proportion of 41 to 1 between the length and breadth of the photographic spectrum, sufficiently indicates it to have been formed by a non-achromatic lens, inclining the surface of the plate forward so as to bring that part on which the more refrangible rays fall nearer the lens than that which receives the less, thus compensating the shortening of the focus for the former rays. And as such (though not stated in respect of this individual specimen) appears to have been Dr. Draper's usual practice; and as in another of his papers he formally recommends it as securing "the great advantage of elongating the total length of the spectrum, and

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though he seems to have read with rather cursory attention what had already been published on this subject in England.—Note added during the printing.

* See Translation in Scientific Memoirs, part 11. (vol. iii.), p. 422.—Ed.

therefore increasing the measures" (Phil. Mag., Dec. 1842, p. 456.), I cannot help observing, that not only is no real advantage gained by such elongation, but the very reverse. For, in a spectrum formed on a surface so inclined, each of the coloured solar images, of whose succession it consists, is not only defective in its individual definition everywhere but at two points in its circumference, but also, instead of being circular as it would be were the surface perpendicularly exposed, is dilated into an ellipse, having its longer axis in the direction of the length of the spectrum, and the overlapping of the contiguous ellipses being necessarily dilated in the same proportion, every abrupt change in the intensity of photographic action becomes softened and smoothed down as it were by being spread over more space, and rendered, by consequence, less salient to the eye than it otherwise would be. In the case of ioduret of silver, indeed, this is not quite of so much moment, because the change of intensity in the action of the spectrum is, as I have shown in Article 129 of the paper already cited, and in Articles 214, 215 of its continuation (Phil. Trans. 1842), so excessively abrupt at the point of union of the blue and violet rays, that it is not in the power of such dilatation materially to mask it. But in innumerable other cases where consecutive maxima and minima occur, these features (*which are always characteristic of the ingredients used*, and on that account especially interesting and important) cannot fail to be grievously marred by thus as it were flattening them down. It is this consideration which decided me, from the very beginning of my inquiries on this subject, always to use an achromatic lens for forming my spectra (unless in cases where the use of *two kinds* of glass is objectionable); and when it has been required to increase the proportion of their length to their breadth for peculiar purposes (as in Article 69 of the above-cited paper), to do so, not by increasing the length, but by diminishing the breadth of the spectrum in the manner there described, so as, at the same time, to preserve the circularity of the sun's image, and also to diminish the overlapping of successive images, and thereby increase the homogeneity of the light at each point of the length. And here I must take occasion also once more to insist (whatever may have been said to the contrary) on the indispensable necessity of using achromatic lenses for photographic practice with the camera obscura *by those who desire perfection*; being myself fully convinced that we have hitherto seen nothing comparable to what photography is capable of performing when the camera shall come to be studied and improved with a view to this especial purpose, as the telescope has been for its own. And I ear-

nestly recommend the subject to our mathematicians and artists as highly deserving their attention.

3. But to return to Dr. Draper's specimen. The ground on which it is projected is *dimmed* by the vapours of mercury settling on the parts where dispersed light has fallen, so as to be rendered much less specular, not only than the unattacked part of the silver surface, but also than the impression of the spectrum itself, which for the most part yields a much more powerful and regular reflexion than any part of the contiguous ground.

4. With respect to the phænomena offered by the ground, it is that of the Daguerrotype in general, and being produced by *mixed* rays, I shall defer its consideration, remarking only at present, that it appears dark in certain lights, white in others, and iridescent with a faint halo-like pink tint in the transition from one light to the others, especially towards its borders.

5. The spectrum itself is extremely remarkable and beautiful. It is divided, on a superficial inspection, into several very distinct compartments, which however, with one exception, are found on close examination to graduate imperceptibly into each other, though with so high a degree of abruptness of transition as may justify our regarding it, in description, as consisting of differently characterized regions. The apparent outlines of these regions are represented in the figure annexed (Plate II. fig. 1), though only a coloured drawing could properly render the general effect and delicate graduation of the several regions (indicated by the letters) into each other.

6. The aspects of the spectrum, as has been observed of the ground, are very different in different lights, assuming the one or the other of two opposite characters, according as it is viewed *by specular reflexion* or by *side light*. These aspects it will be necessary to describe separately.

7. When viewed by specular reflexion, as when viewed at a nearly perpendicular incidence with the back to an open window, or when laid on a table by candle-light and the ground glass or paper shade of a lamp seen clearly by reflexion on the general surface, while no side light is suffered to fall on it; the ground, as above observed, is dark in comparison with the unattacked silver surface. On this ground the terminal regions A, E appear bright and specularly white, though materially less so than the unattacked silver. The region B is black, though not absolutely so: fully as dark however as the ground. This is immediately followed by C, which is white at its outer edges, but very gradually passes inwardly into pale yellow, yellow, and at last terminates in an oval

space, in which the tint rises to a brownish or reddish orange, the terminal edges of the oval being a very little paler than the middle. Immediately, and with absolute suddenness, as if coming out from under this oval, reappears at D the blackness of the region B, but apparently more intense (as if the same quantity of blackness had here been crowded into a narrower space), and with an evident bluish cast. Within the coloured space C, and around a point *m*, which may be regarded as their common focus, the isochromic ovals may be considered as arranged, which, crowded together undistinguishably towards D, elongate in the direction of B, as indicated by the dotted lines. A feeble ash-gray oval train G, barely perceptible, appears to come out again as it were from under D, or to form a kind of tail to it, leaving a slight indentation H on either side, occupied by the specularly white continuation of the portion E. Those who are accustomed to the analysis of such cases will easily perceive here the effect of an abruptly terminal solar image at *m* overlapping and concealing a sudden descent to a minimum, or perhaps to a total absence of photographic action at H.

8. The borders of the elliptically terminated portions A, B, inclose and overpass one another as represented in the figure. For instance, the border of the portion A extends not merely beyond the blackened space B, but even somewhat beyond the extremity of C, thinning away there to an exceedingly delicate line, as does also that of the portion B itself. No traces of these borders however can be followed down the whole length of C.

9. In the order of tints above described as prevailing from the more refrangible end of the spectrum to the maximum of tint *m*, we recognize without difficulty the Newtonian series of colours of the first order of the reflected rings; modified however in its first stages by a cause which seems to have shifted the initial black of that series to a higher point in the scale of thicknesses of the producing film, or to have displaced the whole series by the intrusion of a white commencement. For, the Newtonian reflected tints of the first order are, black, very feeble and hardly perceptible blue; brilliant white; yellow; orange (at which point the series breaks off). And if we suppose these tints (so modified) repeated again in reverse order, and consent to attribute the more intense apparent blackness of the region D beyond that of B to the effect of contrast produced by the greater abruptness of transition in that region (an effect which is very striking in many optical phænomena), the whole spectral impression from end to end will come to be accounted for by a film, homogeneous in its

composition, and varying in thickness according to the law of the curve represented in fig. 2, where the abscissa being the distance from the end of the spectrum to any point in it, the ordinate will represent the thickness of the film at that point, where it will be observed that in using the word *film*, I by no means mean to imply the notion of its *continuity*, as opposed to that of a scattering over the surface of lamellar particles of the requisite thickness.

10. In what manner we are to conceive the above-mentioned intrusion of a white member at the commencement of the series and the further modification of its subsequent members, by dilution to a certain degree of the black, and deadening the extreme brilliancy of the white of that series, seems a point of difficulty which I am not disposed to dissemble. At one time I imagined that it might be accounted for by the intermixture of the transmitted or complementary series of colours reflected from the silver below, on the hypothesis of a high absorptive action in the film itself, which would progressively diminish the influence of such intermixture as the thickness increased, so as to allow the higher tints their full intensity. I am not aware that the subject of the Newtonian rings, as formed by absorptive films, has ever been theoretically treated. But it is easy, without going regularly into it, to see that, since the intensity of any given homogeneous ray is diminished by absorption in geometrical progression while the sum of the thicknesses traversed by it increases in arithmetical—and moreover, since the transmitted series of tints so brought to the eye by reflexion must have traversed (by the theory of their formation) four thicknesses* of the film, while the interfering ray which produces the reflected series has to traverse only two—*therefore* the ratio of intensity of the transmitted to the reflected tint, in the state in which it reaches the eye, will continually diminish in geometrical progression as the tint ascends in the scale, so that the higher orders of tint will be progressively purer, at least so far as this cause is concerned.

But against this there is one obvious objection, viz. that the succession of tints as they stand cannot be allowed to commence with the zero of thickness and go on according to the Newtonian law, unless we assume *that the whole extent of the spectrum, and not merely its terminal portions, as Dr.*

* I here for simplicity lay out of consideration the circumstance that the transmitted tint so conveyed to the eye consists of an infinite series of rays all in one phase of undulation, forming a decreasing geometrical progression of intensity, owing to having traversed respectively 4, 6, 8, 10, &c. thicknesses.

Draper supposes, has been protected from the action of dispersed light: and this is the difficulty which, as I have already stated, I do not wish to dissemble, and which Dr. Draper's negative rays introduced at the ends of the spectrum will not, I conceive, fully get over. The form of the curve, fig. 2, is fully in unison with what I have described and figured respecting the action of the spectrum on ioduretted papers (Phil. Trans. 1842, Article 214—216). And if we have regard to the breadth as well as the length of the spectrum, its law of intensity will be represented by the vertical coordinate of a solid surface represented in front and lateral projection in figures 3, 4, and if each such coordinate be regarded as expressing the proportional thickness of the deposited film at that point, the solid itself will represent on a very exaggerated scale (of perhaps a million to one in height) the actual form of that film, supposed continuous.

11 In this view of the subject the deposited film must be regarded as homogeneous. *i. e.* one and the same chemical substance (whatever that substance be) in its whole extent: granting this, all is one phænomenon regulated by a mathematical expression into which a parameter varying only with the refrangibility of the ray, the time of its action and the intensity of the light, enters. And here I would observe, that the lateral gradation, which is of course due solely to varying intensity, is produced by two very distinct causes, both which it is well in such experiments to bear always in mind as accounting for various singular phænomena of internal ovals and reversed action *along the axis* of the spectrum as exhibited on photographic papers, &c. For, first, at the *edges* of the spectrum there is no overlapping of images, which overlapping from zero at that point goes on increasing to the axis. From this cause originates a progressive scale of intensity proportional to the chord of the sun's circular image, measured in a direction parallel to the axis. And, secondly, it is placed beyond a doubt, both by Mr. Airy's and my own observations, that the central portions of the sun's disc are very much more luminous than the borders, which if it be due (as I have elsewhere stated I conceive it to be) to the absorption of a solar atmosphere extending beyond the luminous surface, would also act on the photographic powers of the rays unequally, and so produce a variation in the lateral rate of photographic action, of which at present we have no measure, but which may possibly be thus brought within our range of investigation, and be rendered obvious by direct examination of the sun's *white* image simply projected on photographic paper, an experiment I propose to try on a favourable occasion.

In reference to the "protecting" rays of Dr. Draper, assuredly the first aspect of the phænomena is favourable to their existence; yet I must observe, that when the spectrum is made to act on ioduretted paper in the mode described in the memoir above referred to, the general ground of the paper is affected by the dispersed light *in the same manner* but *to a less degree* than the spectrum-portion, nor is there any sign of the occurrence about the region of the extreme red and beyond it of that sort of protection from the action of dispersed light which I have described in Phil. Trans. 1840, and to which Dr. Draper refers, as due to the action of *negative rays* stated by me to exist in that region. I have however (as will be seen on referring to Article 90 of that paper) been very cautious in ascribing to them any such decidedly *negative* quality. An effect *of some kind*, conservative of the whiteness of papers variously prepared while under the influence of dispersed light, does undoubtedly take place in the region of the extreme red, and to a certain distance below it. But I have of late learned to regard that effect (which at first I confess had very much the appearance of a negative action) as at least in part a secondary one, and due to a cause which has no existence when silver plates are used, viz. the maintenance of the paper at those particular parts *in a state of superior dryness* to the surrounding parts, by which its sensibility is materially diminished—a certain slight degree of moisture being exceedingly favourable to photographic action in many cases, as I especially find it to be in that of muriated papers overkept, in which the conservative effect is occasionally so remarkable as to extend *over the whole* visible or luminous spectrum. And in one instance (recorded in Article 95 of that paper) we have an instance very much in point, and the more valuable as having been marked as an anomaly at the time, while yet the phænomena of the thermic spectrum, as manifested in the drying of paper, were unknown to myself. For in that article it is recorded that in a certain paper (which had it appears been kept some time before using) the conservative action in question was limited to a circular well-defined solar image beyond the extreme red, and was due, I have now no doubt, to the action of the heat-spot β (see Article 136) either drying the paper, or exerting, it may be, some peculiar effect due to the chemical nature of its heat. For I would wish to be understood as by no means limiting the cause of conservation to this secondary mode of action, or to deny the exercise of an influence which may *so far* be regarded as negative, i. e. as opposed to photographic agency by that portion of the *thermic* spectrum which lies

within and immediately adjoining to the luminous one. The thermic rays belonging to this region possess singular and remarkable properties, of which more presently. And if such conservation be in any way ascribable to *thermo*graphic opposition to *photo*graphic action, it seems not unreasonable to argue from the absence of observable white conjugate images on the discoloured ground in all the very numerous cases where it has been witnessed (with the single exception of that above noticed), that such action is limited to these peculiar thermic rays, and does not extend to the purely calorific ones remote from the spectrum. This subject, however, will require more attention, which on the return of a summer sun I purpose to give it.

To return, however, to Dr. Draper's spectrum. The specular colours of the portion C lower very perceptibly in the scale of tints when viewed in a very oblique incidence, a circumstance agreeing with the analogy of the colours of thin plates. On the other hand, if the incident light be so managed as not to return by specular reflexion to the eye, but merely to illuminate the plate strongly, while all other specularly reflected light is eliminated by a proper adjustment of dark objects in the neighbourhood, the character of the phænomena undergoes a singular and striking reversal. The terminal regions A, E become *dark*, though less so than the unattacked silver; the half black space B becomes strongly white, and the violet-black space D very brilliantly so, showing like frosted silver; while on the other hand the coloured portion C becomes extremely dark, somewhat dull in its aspect, and of a hue which, so far as its great obscurity will permit it to be distinguished as a tint, may be termed steel-gray or bluish. By candle-light a tendency to green may be perceived. This tint, and the blackness it relieves, hardly undergo any variation over the whole space C, except just where it graduates into whiteness at its union with B.

In this succession we recognize, with very slight deviations, the series of Newtonian transmitted rays *undiluted* with the white light which accompanies them when formed by a film of air or glass, and (like those of the reflected series) advanced by the intrusion of a *black* member at their commencement. But it must be noticed as a fact of great moment in their theory, that *they are not the complements of the tints seen by specular reflexion* at the same points of the spectrum. In fact they go no higher than to *one-half* the extent of the series so seen, since the black ring of the first order of transmitted rings corresponds, not to the first black ring, but to the white of the first order, in the reflected series. Here is no doubt

another unexplained difficulty. While on the one hand it is obvious that the dispersed colours arise in a totally different manner from the specular ones, and cannot be regarded as directly connected with them; on the other it does not appear by what superficial structure, by what form and law of distribution of laminæ adhering to it, or by what hypothesis respecting their crystallization, &c., any such colours can be explained. One thing, I think, seems clear, that they cannot be produced by any lamination *parallel* to the surface of the plate; and another, that the laminæ (if any) which do produce them must be disposed indifferently in all azimuths, and (within certain limits) at all angles of inclination to the general surface, since they are seen in all directions.

The absence of the diluting white light which mingles with the pure tints in Newton's transmitted rings, is also a very peculiar character, and would almost incline us to a suspicion of the origin of these dispersed colours being to be sought within the domain of double refraction and polarization, were it not extremely difficult to conceive any structure in which such a mode of production can be realized.

It is not improbable that this obscure part of the subject will receive elucidation from the halo-colours which appear on the white ground, and on the white portions of the spectrum when illuminated by rays nearly approximating to the direction of specular reflexion. In such incidences only they appear, and then only at the borders of the ground where the action of dispersed light has been feeble, and along the region A and the faint train G, which, when the illuminating light is remote from specular incidence, are very faintly illuminated, but, as it approaches that incidence, seem to open out as it were from B and D in brighter light, coloured with faint pink. In the same manner the frosted ground under these circumstances dilates in breadth, so as to cover a much larger portion of the whole plate. Its borders gleam with the pink tint, while its internal portion passes into dark gray, which increases up to the specular incidence; the order of tints in this situation being precisely the first order of colours of the Lunar corona, or of the halos exhibited by a candle seen through a glass breathed on.

These observations may seem unnecessarily minute, but I am persuaded that they are essential to the right understanding of the Daguerreotype phænomenon, when we shall arrive at one. Already we see in our reference of the phases of this phænomenon to the colours of thin plates (with whatever difficulties that reference may be encumbered) a very simple explanation of the change from positive pictures to negative,

by mere continuance of illumination, which has excited surprise in some, but which is a perfectly natural consequence of this view of the matter; while in the opacity or strong absorptive power of the film producing these tints, we see reason to conclude that no increase of thickness which long exposure would give to it, would ever develope in it the complete succession of the Newtonian colours beyond the first or second order; and that in consequence the subsequent longer continued action of the light would fail to produce distinct alternations of positive and negative pictures, though it might give rise to fluctuations of intensity, ending ultimately however in general and total blackness. Many of the intricate phænomena described by Prof. Moser, in his papers above alluded to, will be found divested of much of their enigmatical character by these and similar considerations.

I shall close what I have to say on the subject of Dr. Draper's specimen, by mentioning that about the time when that specimen was produced*, I was myself engaged in forming Daguerreotype impressions of the spectrum. My experiments were made in the last week of July and the first two or three days of August 1842. Want of habitude in the manipulations of the Daguerreotype process (which I had never before executed), and by no means want of sun, prevented my obtaining anything like so fine impressions; but I remained satisfied at that time, and by those experiments,—1st, that the tints of the coloured portion C were those of the Newtonian reflected series, more or less modified by the high refractive power and opacity of the film; 2ndly, that the law of intensity of action in proceeding from end to end of the spectrum, on the Daguerreotype plate, so far as I observed it (for I did not obtain the "negative" terminal portions A or E), is identical with that exhibited on blackened argentine paper (free from nitrate) under iodic influence, and especially the place of the abrupt maximum, which is so characteristic of iodine as a photographic element, proved (by actual measurement) to be precisely the same in both ways of operating. This identity, I should observe, is somewhat difficult to recognize in Dr. Draper's plate, where the maximum would appear to occur somewhat lower on the spectrum; but this, if it be really a point of difference between us, and not merely owing to a different nomenclature of colours, or to the effect of inclining the plate to the incident ray, may and probably has arisen, not from any difference in quality of sunshine in Virginia and England, *but from difference in the law of photographic dispersion in the prisms used.*

* The specimen bears date July 27, 1842.

I further satisfied myself, 3rdly, in the experiments alluded to, that the law of photographic action on a bromuretted silver plate is in like manner identical in respect of intensity, with that exhibited on paper prepared with bromuret of silver in Mr. Talbot's method; the action being carried down in both cases *to*, and considerably beyond, the extreme red ray. But no appearance of protection from the action of dispersed light, which is so conspicuous on the paper, was observed on the silver plate. [This plate I did not mercurialize.]

I had intended to conclude this communication with some remarks in detail on Professor Draper's "Tithonic rays," but having already extended it to I fear an inadmissible length, I must limit myself to throwing in a general caveat against the adoption of *a new name* (and that a most fanciful one) for *an old idea*. The notion of *chemical rays* as distinct from those both of light and heat, is so perfectly familiar to every photologist, as hardly to need insisting on. The extension of chemical action to every part of the spectrum, and considerably beyond the extreme red, is fully demonstrated in my first memoir on this subject; where also the *independence* of the rays in which that action resides, and the simply luminous and colouring ones, is clearly proved by decisive experiments on the absorption of coloured media, perfectly analogous to those brought forward by Dr. Draper, and which I am somewhat surprised he has not noticed, as he has evidently read that memoir.

Should Dr. Draper succeed in establishing those points of analogy between the photographic chemical rays and those of heat, viz. that they are capable of accumulating in or on bodies exposed to them, thence radiating away in the manner of obscure heat (after undergoing such a change as to be no longer capable of traversing colourless glass), but with this marked distinction from such heat, that they are not *conducted* in this their new state by metallic bodies, no one will hesitate to regard him as having attached a new idea to the old one of chemical rays, and as entitled to impose on them a name. But I hope I shall not be considered as undervaluing the really interesting experiments he has brought forward in support of these propositions, if I profess myself as yet unconvinced by them. My own impression is, that there are two distinct kinds of chemical action; exercised, if we choose so to consider the subject, by two distinct and independent classes of rays (not opposed, but different), the one class being those which have been hitherto commonly called chemical, but which, being in this view of the subject not a distinctive name, I shall for the present call them photographic rays;

the other a very peculiar class, which are not only analogous to, but identical with, some at least of the rays conducted by and darted forth from obscurely hot metallic bodies,—which reside (at least in their greatest prevalence) in the less refrangible portion of the spectrum (from the yellow *to*, and perhaps somewhat *beyond*, the extreme red),—which possess chemical properties not participated in at all, or not in anything like so great a degree by the purely calorific rays which lie far beyond the spectrum,—which as they exist where no light is manifested (as in hot mercury), are no way entitled to be called light (for against such a term as *invisible light* I must enter my protest),—and which as they lie in a region of the spectrum, where heat is known to exist abundantly, and produce there the chemical effects above spoken of, apparently by means of a peculiar kind of heat developed by them, *are* entitled to be regarded as heat. To such rays (the existence of which I think I have demonstrated by experiments admitting of no other interpretation), the name parathermic rays may, I conceive, be very properly assigned. And I mention the subject here in order pointedly to mark the difference between *these* rays, in my mode of conceiving them, and the *Tithonic rays* of Dr. Draper, which seem to be *the photographic rays generally*, with certain alleged additional properties which may very properly form the subject of further experimental inquiry.

I have the honour to be, Sir,

Your most obedient,

Collingwood, Jan. 11, 1843. J. F. W. Herschel.

P.S. Added Jan. 20, 1843.—If we generalize our notion of radiating or *Actinic* influence so as to include the several phænomena of light, heat and chemical power operating molecular transformations, and conceive (as M. Becquerel seems disposed to do) that these several manifestations of such influence refer themselves rather to the powers, qualities and limits of the recipient than to original differences in the agent; (a view which may assuredly be taken, whether we regard that agent as an undulation mechanically propagated through a fluid, or in Sir William Hamilton's more refined point of view, as an influence transmitted, wave-fashion, but yet without motion of material particles,) it will still be absolutely necessary to have names distinctive of such very different forms of manifestation as heat, light, and chemical transformation, and rays will still continue to be spoken of as luminous, thermic, chemical, and perhaps by many more epithets, as science advances, without prejudice to any general views we may form as to causes.

XX. *On the Sustaining Voltaic Battery, in reference to some Observations of* Professor Daniell *in the April Number of the Philosophical Magazine,* 1842. *By* F. W. de Moleyns, *Esq., late M.P. for Kerry, M.A., F.G.S., &c.**

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

Do me the favour to insert in your forthcoming Number the following observations on a paragraph in the letter of Professor Daniell, published in the Philosophical Magazine for April 1842. Absence from this country prevented my adverting to it before.

After setting forth the difference between the principles of the constant battery and M. Becquerel's voltaic combination, Mr. Daniell proceeds:—"But, of course, the principles of the construction are independent of form and materials, and are capable of application to flat, square and equal surfaces of the two metals as well as to concentric arrangements. They admit also of the employment of different metals and of different electrolytes. They are *not* changed by placing the zinc on the *outside* instead of the *inside* of the copper, nor even by altering the name of the constant battery to that of the sustaining battery." Now, as the designation "Sustaining" was given by me to my battery, upon its introduction in 1836, and distinguishes it fully as much as the term "Constant" does Prof. Daniell's; and as it might be inferred from the above extract that the principles of the two batteries are not only identical, but that the "Sustaining" is a mere modification of the "Constant," I feel myself called upon to protest in the strongest manner against the correctness of such a conclusion.

It is incorrect, because, assuming Prof. Daniell to have truly stated his own principles, there is not one principle in my combination similar to his, and this difference in *principle* is supported by the very wide difference in *construction*. A few sentences will show the distinction.

1st. My battery is *not* constructed "on the principle of a *central* disposition of the active metal with regard to the conducting surface†," but on the very different principle of *circumferential* disposition, which offers the advantages of proxi-

* The publication of this letter is urged upon us by the author as an act of justice which it would be unfair in us to refuse.

Mr. De Moleyns alleges that we "have published papers containing injurious remarks and insinuations," and that "it becomes a point of justice to afford him an opportunity of protecting himself from the conclusions which would be drawn were he to remain silent".—Ed.

† See Daniell's Chemical Philosophy, sect. 737–705.

mity of the two metals, as well as of the preservation of the same proximity during action, and which cannot be made by any straining, to involve the principle of central forces as explained by Prof. Daniell *.

2ndly. My battery is *not* founded upon the principle of *equal* action being produced from a minute as from a large surface of the active metal, so long as the inactive metal remains unchanged †, but upon the contrary principle of presenting an active surface, at least one-fourth of the inactive, *less* than that being found to diminish the action.

3rdly. I do *not* advocate the use of *acidulated* solutions, or of *amalgamation* of the active metal; on the contrary, I support the principle of obviating local action, and of preventing injurious combinations, by employment of a solution of chlorhydrate of ammonia (*par excellence*) in contact with the active metal.

4thly. I do *not* assert one of my principles to be that of "*perfectly* preventing the mixture of the liquids on the opposite sides of the diaphragm employed ‡;" on the contrary, I encourage mixture to a certain extent, for it unquestionably promotes electrolysis.

5thly. I did *not* adopt a diaphragm "in order to prevent a deposition of active metal on the conducting surface §," inasmuch as I used not one fluid, but two, to which latter case that principle does not apply. One of my reasons for using a diaphragm was to prevent the too intimate mixture of the fluids employed.

So much for the *subordinate* principles of construction of both combinations. With respect to the *general* principle of the adoption of two fluids, two metals, and a diaphragm, with a view to the obtainment of constancy of effect,—that principle having been discovered and promulgated by M. Becquerel in the year 1829, and Porrett's experiments with diaphragms being previously published,—I believe I was at perfect liberty, in the construction of the sustaining battery, to avail myself of any aid I might require from investigations so well known, without subjecting myself to the suspicion of borrowing from a subsequent invention, an invention of which I was utterly ignorant until after my combination was completed.

I am, Gentlemen,

Your most obedient Servant,

London, Dec. 1842. F. W. De Moleyns.

* See Daniell's Chemical Philosophy, sect. 701, 705.

† Ibid. latter part of sect. 701.

‡ See Prof. Daniell's letter in Philosophical Magazine for April 1842.

§ See same letter, Philosophical Magazine for April.

XXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxi. p. 228.]

Nov 17, 1842. THE following papers were read, viz.—

1. Postscript to a paper "On the Action of the Rays of the Solar Spectrum on Vegetable Colours." By Sir John Frederick William Herschel, Bart., F.R.S., &c.

An account is here given of some additional facts illustrative of the singular properties of iron as a photographic ingredient, and also of some highly interesting photographic processes dependent on those properties, which the favourable weather of the summer has enabled him to discover. The author also describes a better method of fixing the picture, in the process which he has denominated the *Chrysotype*, than that which he had specified in the latter part of his paper. In this new method the hydriodate is substituted for the hydrobromate of potass; and the author finds it perfectly effectual; pictures fixed by it not having suffered in the smallest degree, either from long exposure to sunshine or from keeping.

He next considers the class of processes in which cyanogen, in its combinations with iron, performs a leading part, and in which the resulting pictures are blue; processes which he designates by the generic term *Cyanotype*. Their varieties appear to be innumerable, but one is particularly noticed, namely, that of simply passing over the ammonio-citrated paper, on which a latent picture has been impressed, very sparingly and evenly, a wash of the solution of the common yellow ferrocyanate of potass. As soon as the liquid is applied the negative picture vanishes, and is replaced, by very slow degrees, by a positive one, of a violet-blue colour on a greenish-yellow ground, which, at a certain moment, possesses a high degree of sharpness, and singular beauty and delicacy of tint. From his further researches on this subject he deduces the following conclusions: first, that it is the heat of the rays, not their light, which operates the change; secondly, that this heat possesses a peculiar chemical quality, which is not possessed by the purely calorific rays outside of the visible spectrum, though far more intense; and thirdly, that the heat radiated from obscurely hot iron abounds especially in rays analogous to those of the region of the spectrum above described.

The author then describes the photographic properties he has discovered to belong to mercury, a metal which he finds to possess, in an eminent degree, direct photographic susceptibility*.

2. "Observations de la variation de la déclinaison et intensité horizontale magnétiques observées à Milan pendant vingt-quatre heures consécutives, le 22 et 23 Juin, le 20 et 21 Juillet, le 26 et 27 d'Aout, le 21 et 22 Septembre, et le 19 et 20 Octobre, 1842," rapportées par Robert Strambrecchi, premier élève adjoint.

* This paper will appear entire in a future Number of the Philosophical Magazine.—EDIT.

A letter was also read from Sir John F. W. Herschel on the subject of Photography, addressed to S. Hunter Christie, Esq., Sec. R.S.

November 24.—The following papers were read, viz.—

1. "On certain improvements on Photographic Processes described in a former communication." By Sir John Frederick William Herschel, Bart, K.H., F.R.S., &c., in a letter to Samuel Hunter Christie, Esq., Sec. R.S. Communicated by Mr. Christie.

The present memoir, which is a sequel to the last by the same author, is accompanied by a series of photographic impressions illustrative of the chrysotype, cyanotype, and other processes formerly described by him. Some improvements which he has introduced into these processes are given, together with a few remarks on some other points treated of in the former paper, in relation to the influence of *thermic rays* as distinct from *calorific rays*; the former being rays, which in the spectrum accompany the red and orange rays, which are also copiously emitted by heated bodies short of redness, and which are distinguished from those of light by being invisible. The author thinks they may be regarded as bearing the same relation to the calorific spectrum which the photographic rays do to the luminous one, and would propose to designate them by the term *parathermic rays*. He conceives that these may be the rays which are active in producing those singular molecular affections determining the precipitation of vapours in the experiments of Messrs. Draper, Moser, and Hunt, and which will probably lead to important discoveries as to the intimate nature of those forces, resident on the surfaces of bodies, to which M. Dutrochet has given the name of *epipolic forces*.

2. "Boring Register, Bow Island, South Pacific." By Captain Edward Belcher, R.N., communicated by Captain Beaufort, R.N., F.R.S.

The results of the boring operations carried on in this island are here given, as well as the register of the daily proceedings, under the particular superintendence of Mr. Thomas Pass, acting master of H.M.S. Sulphur. The depth reached was 45 feet, when the auger broke, and no further progress could be made.

November 30.—*Anniversary Meeting.*—The President addressed the meeting as follows:—

GENTLEMEN,

I MUST commence my address to you by the expression of my regret that my absence from England at this period of the last year prevented my being then able to meet you at your Anniversary. The gratitude which it has behoved me to intimate to my Council on former occasions for their assistance in the discharge of my presidential duties, it is more than ever necessary for me now to feel, as it was that assistance that rendered my absence no real detriment to the Society.

During that absence, an event took place to which I am bound to refer,—I allude to the visit to this city of the sovereign of another country at the time of the auspicious baptism of His Royal Highness the Prince of Wales. His Prussian Majesty was pleased to

join our Society. At this I heartily rejoice, as I believe it to be a happy omen for mankind when those who are placed in exalted situations show their sympathy with scientific pursuits. I congratulate the Prussian nation, that her sovereign has taken so early an opportunity of countenancing science, and of declaring his opinion that the natural philosopher is a friend to good government, to order, and to civilization.

The pleasure experienced by you on this occasion was enhanced by the presence within these walls of Baron Humboldt, who accompanied his Majesty. It is very seldom that we can expect to see among us any of our Foreign Associates. It was therefore doubly gratifying to receive, together with his sovereign, the distinguished philosopher who had travelled over so large a portion of the globe in the pursuit of every branch of knowledge.

Since I last addressed you, two years ago, a great degree of success has attended the expedition of Captain Ross to the Antarctic Region. I congratulate you, Gentlemen, on the results already derived from an expedition which originated in a joint application to Government from your Council and the British Association. I rejoice that a British officer has had the honour, not only of making most important scientific researches, but also of approaching much nearer to the Southern Pole than any one had done before him, and of discovering a new Iceland and a new Hecla, more gigantic than the arctic volcano.

With respect to the magnetic observatories, I have the gratification of informing you that they are to be continued for three more years, in hopes of making the information to be obtained from them more extensive and more accurate. The consent to this continuance was granted by Sir Robert Peel: a continuance of the scientific measure of one minister by the statesman who had superseded and succeeded him. This is a gratifying circumstance, as proving that, as we hope and believe that British patriotism belongs to all parties, so the love of science also belongs to all, or rather that in scientific pursuits there is no party feeling and no party jealousy. I must add, that on the present occasion, the application of the Council of the Royal Society was seconded by M. Brunow, the ambassador of the Emperor of Russia; thus showing that nations are ready to testify that any great acquisition of physical knowledge is a common object to the whole human race.

The hopes that the Expedition to the Niger might be productive of important additions to our stores of science, as well as great results to the highest interests of humanity, have been unhappily in a great measure disappointed. At the same time the hopes of the scientific naturalist have not been entirely vain, for I am informed by Mr. Gray that many new species of birds and other animals have been brought to England from Fernando Po and the mouth of the Niger.

Your Council, Gentlemen, have taken into their consideration the great importance that microscopical researches have always possessed, and the still greater influence upon science that they are

now beginning to exercise, in the hands of Mr. Owen and others, as well as the extraordinary perfection to which the instrument itself is now brought. They have come to the conclusion that it is highly expedient that we should ourselves possess the means of repeating and verifying the experiments brought before our notice, as well as instituting new branches of inquiry. We have therefore thought it expedient, by summoning competition to our aid, to endeavour to obtain one of the best microscopes that can be constructed. Indeed we feel sure, that, independently of the liberal price that we have offered, there is no optician who would not feel highly gratified on seeing within these walls an instrument constructed by him.

The room, Gentlemen, in which we are met, has had some changes made in the pictures which adorn its walls. In consequence of these changes you will see, in addition to those portraits to which you are accustomed, the likeness of one of the most distinguished of our body; of one who was equally eminent in natural philosophy and in archæology. Our posterity, Gentlemen, will probably hereafter be at a loss whether to admire Dr. Young most in his pursuits of natural knowledge, or in his discovery of the key to the greatest mystery of bygone ages,—the hieroglyphical writing of the Egyptians.

You will not be less pleased to see another portrait of a venerable philosopher still spared to us—of that great and original chemist, Dr. Dalton.

I have to congratulate you also on the possession of the bust of a lady whose acquirements are an honour to her sex and to her country; and I feel sure that the likeness of Mrs. Somerville, from the hand of our lamented Chantrey, will ever be highly prized by the Royal Society.

In addition to these ornaments to our Apartments, since I addressed you in our Anniversary Meeting of 1840, I must not pass over the portrait of Mr. Dollond, to whom the astronomer is so much indebted for his improvement in the art of constructing telescopes; and I should be wholly inexcusable if I omitted the valuable picture given to us by Mr. Vignolles, and representing the prince of English science, the immortal Sir Isaac Newton.

I am happy to state that the Royal Society has not, during the past year, had to lament the death of any one of her Foreign Members. We could not reasonably hope that such should be the case among her British Fellows. I shall now, Gentlemen, conclude, as usual, by a short account of some of the more remarkable men, whether for scientific research, or for public services, whom the Royal Society has had the misfortune to lose since last November.

Among the deceased Fellows of the present year, we have to lament the loss of one of the most eminent surgeons and physiologists of our times—one whose investigations and discoveries have shed a new light on that most intricate part of the human organization—the Nervous System.

Sir Charles Bell, K.H., F.R.S. L. and E., &c., the youngest son of the Rev. W. Bell, of the episcopal church of Scotland, was

born at Edinburgh in the year 1778. While a mere youth, he was instructed in the elements of anatomical science by his brother Mr. John Bell (himself a distinguished surgeon and anatomist), and at a very early period he published the first part of "Plates of Dissections;" a work alike remarkable for the fidelity of the anatomical details, and the spirited style of the illustrations from the pencil of the author.

In 1799, Mr. Charles Bell was admitted a member of the Royal College of Surgeons of Edinburgh, and soon afterwards was appointed one of the surgeons of the Royal Infirmary in that city, where he acquired a high reputation as a skilful and dexterous operator.

In 1806, he removed to London; and by his own unaided exertions, established himself as a lecturer on Anatomy and Surgery. He was subsequently associated with Mr. Wilson in the celebrated anatomical school of Great Windmill Street, and speedily became one of the most popular and effective lecturers in the surgical schools of London; although at that period, Cline, Cooper, Abernethy, and other eminent men, were in the zenith of their fame as professional teachers.

He was elected Surgeon to the Middlesex Hospital in 1812.

A few years afterwards he was appointed Professor of Anatomy and Surgery to the Royal College of Surgeons of London, in which capacity he delivered a series of lectures, which excited in an extraordinary degree the interest and attention of the profession, the theatre of the College being crowded to the conclusion of the course.

Immediately after the battle of Waterloo, Mr. Charles Bell, with that humanity and zeal for the pursuit of professional knowledge which marked his character, proceeded to Brussels, and tendered his assistance to the wounded soldiers in the hospitals of that city; and after his arrival he was incessantly engaged for three successive days and nights in the operations and dressings of upwards of 300 cases.

In 1826, Mr. Charles Bell was admitted a Fellow of the Royal Society.

On the institution of the London University College, in 1828, Mr. Charles Bell was chosen Principal of the Medical School; and he delivered the opening lecture in that department of the College, and also a course of lectures on Physiology.

On the accession of William IV. to the throne, Mr. Charles Bell, together with a limited number of other men of distinguished scientific attainments, received the honour of knighthood.

A "Treatise on Animal Mechanics," composed by Sir Charles Bell for the Society for the Diffusion of Useful Knowledge, being the substance of some of the lectures which he had delivered before the College of Surgeons, contained so powerful and lucid an exposition of the proofs of creative design, as exemplified in the structure of the human frame, that our late President, Mr. Davies Gilbert, was led to select the author as one of the Bridgewater Essayists. "An Essay on the Hand, its mechanism and its vital endowments as

evincing design," is the title of the admirable volume which Sir Charles Bell, in accordance with the provisions of the appointment, contributed to those celebrated essays.

Sir Charles Bell, in conjunction with Lord Chancellor Brougham, also published "Illustrations of Dr. Paley's Evidences of Natural Theology."

In 1836, he accepted the Chair of Surgery in the University of Edinburgh, to which he was invited by the unsolicited and unanimous vote of the patrons of that institution; and he left London to place himself at the head of the profession in his native city. In this new sphere of usefulness he continued to pursue with undiminished ardour the cultivation of surgery and physiology until his death, which took place on the 29th of April, 1842, at Hallow Park, in Worcestershire.

With this brief sketch of the professional career of Sir Charles Bell, I proceed to notice those original and important investigations into the nature and functions of the nervous system, upon which his high reputation as a physiologist is based, which entitle him to be ranked among the most distinguished Fellows of this Society, and for which he was deservedly awarded the first Royal Medal we had to bestow.

The earliest contribution of Sir Charles Bell to our Transactions was in 1821, "On the Nerves, giving an account of some experiments on their structure and functions, which lead to a new arrangement of the system." This was followed by other essays on the same subject, which were severally published in the Philosophical Transactions for 1822, 1823, 1826, 1829, 1832, 1834, 1835, and 1840.

In the last communication, entitled "On the Nervous System," the author gives a condensed view of his investigations and discoveries, the result of more than thirty years of indefatigable labour and research.

As long since as 1806, in the first edition of his beautiful work "On the Anatomy of Expression in Painting," we perceive the germ of those original views of the nervous system, which it was the labour of his life to elucidate and establish. "If," he observes, "we had but a perfect knowledge of the functions of the nerves, they would on all occasions inform us of the cause of those actions which now appear to us so inexplicable." And here I may observe, that the drawings which illustrate this work are in the first style of art, and show, that had the author chosen painting as a profession, he would have attained a distinguished rank as an artist.

In 1811, in a small work entitled "An Idea of a new Anatomy of the Brain, submitted for the observation of his friends, by Charles Bell, F.R.S.E," he distinctly enunciates those original opinions, which, modified and extended by subsequent investigations and discoveries, have led to those enlarged and philosophical views of the phenomena of the nervous system, which have so largely contributed to the advancement of physiological science.

In short, whatever we may owe to the genius and labours of other

men in this field of research, the discovery of the grand fundamental principle upon which a correct knowledge of the functions of the nervous system depends, is unquestionably due to Sir Charles Bell. He was the first to ascertain, not by accident, but by careful and laborious dissections and experiments, and by a cautious induction from the phenomena which his talents and unwearying industry enabled him to develope, that "the nerves which we trace in the body are not single nerves possessing different powers, but are bundles of different nerves whose filaments are enclosed in one common sheath, but which are as distinct in function as they are in origin; that they depend for their specific attributes on the nervous masses to which they are severally attached; that the spinal nerves arising from the lateral and anterior columns of the medulla spinalis convey the power of motion, while the nerves arising from the posterior strands communicate the faculty of sensation to the several parts of the body to which they are distributed." The nerves which arise from the middle and upper columns of the spinal marrow, Sir Charles conceived to be designed for the act of respiration; and these he termed the "*system of respiratory nerves.*"

Having thus established the principle by anatomy and experiment, that the nerves possess distinct functions in correspondence with their origin from different parts of the brain and spinal marrow, Sir Charles Bell followed up his inquiries by collecting such pathological facts as served to illustrate and confirm the opinions he had advanced; and our Transactions are enriched by numerous memoirs relating to this most important subject. His essays on the nerves of the face in health and disease are of the deepest interest, and their practical value cannot be too highly estimated. In fact, the great advancement which has been made of late years in our knowledge of the nature and treatment of the diseases of the nervous system, is mainly attributable to the labours and discoveries of Sir Charles Bell*.

* A list of Sir Charles Bell's contributions to the Philosophical Transactions is subjoined.

1. On the Nerves; giving an Account of some Experiments on their Structure and Functions, which lead to a new arrangement of the System. (Phil. Trans. 1821, p. 398.)

2. Of the Nerves which associate the Muscles of the Chest, in the actions of Breathing, Speaking and Expression; being a continuation of the paper on the Structure and Functions of the Nerves. (Ibid. 1822, p. 284.)

3. On the Motions of the Eye, in illustration of the Uses of the Muscles and Nerves of the Orbit. (Ibid. 1823, p. 166.)

4. Second part of the paper on the Nerves of the Orbit. (Ibid. 1823, p. 289.)

5. On the Nervous Circle which connects the voluntary Muscles with the Brain. (Ibid. 1826, Part II. p. 163.)

6. On the Nerves of the Face; being a second paper on that subject. (Ibid. 1829, p. 317.)

7. Of the Organs of the Human Voice. (Ibid. 1832, p. 299.)

8. On the Functions of some parts of the Brain, and on the relations between the Brain and Nerves of Motion and Sensation. (Ibid. 1834, p. 471.)

In private life this eminent man was distinguished by the suavity and simplicity of his manners, by his elegant tastes, and domestic virtues*.

Mr. JAMES IVORY was the son of Mr. James Ivory, watchmaker in Dundee, and was born in that town in the year 1765. He received his elementary education at the public schools of Dundee, and in the year 1779, was sent to the University of St. Andrews, where, in the period of four years, he went through a course of Languages, Science and Philosophy, entitling him to the Degree of Master of Arts, which was afterwards conferred on him. While at this University he was distinguished for his attainments in Mathematics, to the study of which branch of science he had, even at this early period of his life, particularly applied himself, under the able instruction of the Rev. John West, at that time assistant to the Professor in the University. It reflects equal credit upon the pupil and the instructor, that for this gentleman Mr. Ivory ever after entertained the highest regard.

Being intended for the Church of Scotland, he now commenced his studies in theology, and in the prosecution of them remained two years at St. Andrews, after the completion of his course of Philosophy. He then removed to the University of Edinburgh; and it is not a little remarkable that he should have done so with Leslie, who had been his fellow-student at St. Andrews. At Edinburgh, he received his third year's theological instruction, necessary, by the regulations of the Scottish church, to qualify him for admission as a clergyman. His studies in divinity were not, however, prosecuted farther; for immediately on leaving the University of Edinburgh, he was, in 1786, appointed assistant-teacher in an academy then instituted in his native town of Dundee, for the purpose of instruction in mathematics and natural philosophy. Having remained in this situation three years, he entered upon a totally different career, becoming a partner in, and the manager of a Flax-spinning Company, which had its mills at Douglastown in Forfarshire, and which assumed the name of James Ivory and Company.

Though now engaged in commercial and manufacturing pursuits, Mr. Ivory still devoted every moment of leisure to his favourite object, the prosecution of mathematical investigations. Living in a secluded part of the country, he was debarred from the advantages of access to libraries and the society of men of science, which a more favoured locality might have afforded him; but this obstacle to the enlargement of his knowledge was overcome by the force of his genius and his powers of application. With a sound knowledge of the geometry of the ancient and of the modern mathematics of his own country, he had already possessed himself of the methods

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9. Continuation of a paper on the Relations between the Nerves of Motion and Sensation, and the Brain; more particularly on the Structure of the Medulla oblongata and the Spinal Marrow. (Phil. Trans. 1835, p. 255.)

10. On the Nervous System. (Ibid. 1840, p. 245.)

* An excellent account of the life and writings of Sir Charles Bell will be found in Pettigrew's Medical Portrait Gallery, vol. iii.

and discoveries of the continental mathematicians, at that time almost wholly unknown in Britain; and he early led the way in that path which he afterwards followed with unrivalled success.

His earliest memoir, read before the Royal Society of Edinburgh, on the 7th of November 1796, and published in its Transactions, shows, not only that at this time he was well acquainted with the works, and possessed the methods of the most celebrated of the continental writers, but that he could advance independently in the track which they had discovered and so successfully pursued. This memoir, entitled "A New Series for the Rectification of the Ellipse, together with some Observations on the Evolution of the Formula $(a^2 + b^2 - 2ab \cos \varphi)^n$," besides displaying considerable analytical skill in the accomplishment of its immediate object, shows that the solution of the highest class of physical problems had already engaged the author's attention.

Two other memoirs, communicated by Mr. Ivory to the same Society, one in 1799, "A New Method of resolving Cubic Equations," and the other in 1802, "A New and Universal Solution of Kepler's Problem," both indicate great originality of thought and powers of investigation. The approximation which he gives in the latter memoir for the determination of the excentric anomaly is remarkable for its simplicity, universality, and accuracy.

At this period, Mr. Ivory was in correspondence with Professor Playfair, Mr. Leslie (afterwards Sir John Leslie), Mr. Wallace and Mr. Brougham (now Lord Brougham), and with these eminent persons his intercourse was ever after continued until interrupted by the death of one of the parties. To the well-founded recommendation of Lord Brougham he was indebted for the grant of a pension of £300 per annum, in 1831, by King William IV.

Released from the anxieties of mercantile speculations by the dissolution of the company of which he had been the manager, he, in 1804, applied for, and immediately obtained, one of the Mathematical Professorships in the Royal Military College at Marlow (afterwards removed to Sandhurst). During the time that he was connected with this institution, he acquired the esteem and regard of the authorities of the College, of his colleagues, and of his pupils. In the discharge of his public duty he appears to have been altogether exemplary; and he was universally considered to be one of the best and most successful instructors that had ever been connected with the College.

He now became better known in the scientific world, and while he discharged the important duties of his Professorship to the advantage of the College and the advancement of its character, he communicated to the public many important memoirs on various scientific subjects, which appeared in the Philosophical Transactions, in Leybourn's Mathematical Repository, Maseres's Scriptores Logarithmici, and the Supplement to the sixth edition of the Encyclopaedia Britannica.

About the year 1816, his health began to give way under the confinement consequent upon close application to his professorial

duties, and devoted attachment to scientific inquiry; and he was compelled by bad health to resign his Professorship. The estimation in which he was held by the authorities of the College cannot be more conclusively shown than by the fact, that, when disabled by ill health from performing his arduous duties, the Governor and the Commissioners of the College recommended and procured the retiring pension to be given to him, some years before he had completed the period of service which the regulations of the War Office at that time required. He now took up his residence in London, and in this metropolis or its environs he spent the remainder of his days, living always in great retirement.

Disengaged from professional duties, though still suffering in health, he now devoted his whole time and all the energies of his powerful mind to the investigation and elucidation of various mathematical problems of the highest order; and the result of his inquiries were given to the world in numerous elaborate memoirs, many of the most important of which, it is gratifying to reflect, adorn the volumes of our Transactions. It is no less gratifying to feel that this Society was at the time fully alive to the value of these communications, by awarding to their author, on successive occasions, the highest honours in its power to bestow. In 1814, Mr. Ivory received the Copley Medal "for his various Mathematical communications printed in the Philosophical Transactions."

In 1826, one of the Royal Medals was awarded to him "for his Paper on Astronomical Refractions, published in the Philosophical Transactions for the year 1823, and his other valuable papers on Mathematical subjects." And again in 1839, he received one of the Royal Medals "for his Paper on the Theory of Astronomical Refractions, published in the Philosophical Transactions for 1838," which paper was the Bakerian Lecture for the year.

If Mr. Ivory's rank among the mathematicians of his age could be assigned independently of his communications to the Royal Society, he must still occupy a distinguished place, not only among those of his own country, but of Europe. It was, however, by the communications with which he has enriched our Transactions, that he gained the great scientific reputation which he enjoyed, and it is with them also that we are more immediately concerned.

These papers may be classed under eight different heads; for although several of them are closely related in regard to their physical objects, yet the nature of the mathematics employed in them is so different, that we should do injustice to his reputation if we arranged them under one head.

The first of these is the investigation of the attraction of homogeneous ellipsoids of the second order upon points situated within or without them, printed in the Transactions for 1809. This paper contained the celebrated theorem by which the attraction of an ellipsoid on a point exterior to it, is made to depend upon the attraction of another ellipsoid upon another point interior to it; the latter investigation being, as is well known, comparatively easy. The solution of the more difficult case had been reduced to a form

nearly equivalent to this by Laplace, but his process was troublesome; that by Mr. Ivory is remarkably simple and elegant. Although this transformation constitutes the most valuable part of the paper, it would be wrong to omit to state that the developments which it contains, on the investigation of the attraction in the simpler case, are highly ingenious, and exhibit a perfect command of analysis.

The second subject is the criticism upon the method used by Laplace in the third book of the 'Mécanique Céleste,' for the computation of the attraction of spheroids of any form differing little from spheres, and the substitution of a method purely analytical for some of Laplace's operations which are founded on a geometrical consideration. The papers which contain Mr. Ivory's remarks on these subjects are two papers and an appendix in the volume for 1812, and one in that for 1822. The remarks on Laplace's theory adverted to two points. One of these was the faultiness of his reasoning as relates to the evanescence of the attraction of the particles included between the spheroidal and a spherical surface when the attracted particle was brought very near to the surface. The other was a limitation of the generality of Laplace's assumption for the form of the function expressing the distance between the sphere and the spheroid, to a rational function of the coordinates of each point. With regard to the first of these subjects, it seems impossible to deny that Laplace had, in the greater part of his investigation, left the interpretation of his suppositions in some obscurity; and Mr. Ivory has, with remarkable acuteness and analytical skill, exposed the defects of Laplace's investigation on *his* interpretation of the suppositions. Yet we must observe that the limitation expressed by Laplace ("supposons de plus que la sphère touche le sphéroide, &c.") appears to be entirely overlooked by Mr. Ivory, and that this limitation, when its effects are fairly examined, completely removes the objection. As to the second subject, it is, we believe, allowed by Mr. Ivory himself, that there is no failure in the investigation if the function for the distance between the sphere and the spheroid, though not explicitly rational, admits of being expanded in a converging series whose terms are rational; the only case undoubtedly that can ever occur in physical application. The analytical process which Mr. Ivory substituted for a part of Laplace's is extremely beautiful.

To show the estimation in which Mr. Ivory's talents and labours were held by Laplace himself, we may here quote a remark from Sir Humphry Davy's Address in 1826, on the award of the Royal Medal to Mr. Ivory. "I cannot pretend," says our, then, distinguished President, "to give any idea of the mathematical resources displayed in the problems, and which even the most accomplished geometer could not render intelligible by words alone; but I can speak of the testimony given by M. de Laplace himself in their favour. That illustrious person, in a conversation which I had with him some time ago on Mr. Ivory's first four communications, spoke in the highest terms of the manner in which he had treated his subject; one, he said, of the greatest delicacy and difficulty, requiring

no ordinary share of profound mathematical knowledge, and no common degree of industry and sagacity in the application of it."

The investigations to which we have just alluded are those upon which Mr. Ivory's European reputation as a consummate mathematician was principally founded; and deservedly so. It is no small praise, even at the present time, to assert of any mathematician, that he thoroughly understands the remarkable investigations of Laplace applying to the attractions of spheroids; and it would be still greater to assert that he is able to substitute a new, clear, and elegant process, in place of one portion which seems doubtful and indirect. But at the time when these papers were written (1808 and 1811) the merit was vastly greater than it would be now. Very few English mathematicians could then read with ease an investigation written in the notation of the differential calculus; scarcely any could understand a process of partial differentials; and probably not another person in the kingdom besides Mr. Ivory had read that part of the Mécanique Céleste. In acknowledging that Mr. Ivory most justly earned the reputation which he acquired (and our remarks above, detracting from the necessity of his criticism, do not in the least detract from its singular skill and command of mathematics), we must not omit also to acknowledge, that to his example we owe, in no inconsiderable degree, that direction of mathematical study which has enabled England, at last, to compete in the field of mathematical science with the other nations of Europe, to which she was during a long interval inferior.

The third subject is the investigation of the orbits of comets. Mr. Ivory's method, printed in the Transactions for 1814, is founded on the supposition that the orbit is a parabola, and it tests the trial-assumption of the distance of the comet by the well-known expression for the time depending on two radii vectores and the chord joining them. Although the analysis is elegant, there is not much of originality in this process.

The fourth subject is the investigation of atmospheric refraction. The papers relating to this are contained in the volumes for 1823 and 1838. The former of these proceeds solely on the supposition that the temperature of the air (as entering into the factor which connects the density with the elasticity) decreases uniformly for uniform increase of elevation. The investigation is not remarkably different from those of other writers on the theory of astronomical refractions. The latter contains the effects of adding to the expression for the density of the air resulting from the first supposition, a series of terms following a peculiar law which make the expression perfectly general for all laws of temperature, and which at the same time offer great facilities for mathematical treatment. The whole investigation deserves particular notice as a beautiful instance of mathematical skill. Considerable labour was also bestowed by Mr. Ivory, in these papers, on the ascertaining, from the best accredited experiments, of the values of the constants which enter into different parts of the formulæ.

A fifth subject was treated by Mr. Ivory in elaborate papers in

our Transactions for 1824, 1831, 1834, and in a portion of a paper in the Transactions for 1839. The object, in these papers, was to show that the method in which the equilibrium of fluid bodies has been treated by mathematicians is defective, one additional equation being, in Mr. Ivory's views, logically necessary, although he allows that its introduction produces no change of results in the case which he has investigated at great length, namely, that of a homogeneous fluid. The Royal Society have conceived that the acknowledged uncommon abilities of Mr. Ivory, and the great attention which he had given to this particular subject, made it almost imperative on them to afford every facility which their Transactions could give to the elucidation of his views, more especially as the logical foundation of the theory had scarcely been canvassed to the same extent as that of many other physico-mathematical theories. At the same time they think it necessary, in adverting to this particular theory, to remark, that no other mathematician has agreed with Mr. Ivory in the necessity of his new equation.

While Mr. Ivory still had the subject of the equilibrium of fluids in his consideration, the very remarkable discovery was announced, by MM. Jacobi and Liousville, that it is theoretically possible that a homogeneous ellipsoid with three unequal axes, revolving about one of these axes, may be in equilibrium. In a paper in the Transactions for 1838, Mr. Ivory has with great elegance demonstrated this theorem, and has given, with greater detail than its authors had entered on, several statements regarding the limitations of the proportions of the axes. This may be regarded as the sixth subject.

A seventh subject, the Theory of Perturbations, was treated in papers in the Transactions for 1832 and 1833. The first of these is a treatment of the theory of the variation of the elements, giving no new result, but simplified, in the author's opinion, by the introduction of the area described upon the planet's moving orbit. The second relates merely to the expansion of the perturbing function, in which, by departing in some degree from the usual process, Mr. Ivory conceived that he had given greater facilities for the developments to the higher order of excentricities and inclinations.

An eighth subject, which we have reserved for the last, as containing nothing of a physical character, is the Theory of Elliptic Transcendents, treated in the Transactions for 1831. We are not aware that anything important is added to the theory in this paper, although a new form is given to some of the demonstrations.

The great scientific reputation which Mr. Ivory had established by these and other memoirs not communicated to the Royal Society ensured his election into this Society in 1815, and into many of the other Scientific Societies of this country and of the Continent. He was an Honorary Fellow of the Royal Society of Edinburgh, an Honorary Member of the Royal Irish Academy, and of the Cambridge Philosophical Society; Corresponding Member of the Royal Academy of Sciences of the Institute of France, of the Royal Academy of Sciences of Berlin, and of the Royal Society of Göttingen.

In 1831, the Hanoverian Guelphic Order of Knighthood was con-

ferred on him by King William IV., and it was intimated that he might also receive the British Knighthood, but this he declined, as the title would have been inconsistent with his circumstances. He had, however, as has already been stated, a pension of £300 per annum subsequently conferred on him by His Majesty. In 1839, the University of St. Andrews conferred on him the Degree of Doctor of Laws.

Although his health had been early impaired by his close application to scientific investigation, he never allowed himself to be unoccupied, but was constantly engaged in his researches to the period of his last illness. In the end of last year his health became seriously impaired, and after an illness of several months, but retaining his faculties to the last, he died on the 21st of September of the present year, aged 77. He was never married*.

Aylmer Bourke Lambert, Esq., was born at Bath on the 2nd of February, 1761. He was the son of Edmund Lambert, Esq., of

* The contributions of Mr. Ivory to the Philosophical Transactions are the following:—

1. On the Attractions of Homogeneous Ellipsoids. (Phil. Trans. 1809, p. 345.)
2. On the Grounds of the Method which Laplace has given in the second chapter of the third book of his Mécanique Céleste for computing the Attractions of Spheroids of every description. (Ibid. 1812, p. 1.)
3. On the Attractions of an extensive class of Spheroids. (Ibid. 1812, p. 46.)
4. A New Method of deducing a first Approximation to the Orbit of a Comet from three Geocentric Observations. (Ibid. 1814, p. 121.)
5. On the Expansion in a series of the Attraction of a Spheroid. (Ibid. 1822, p. 99.)
6. On the Astronomical Refractions. (Ibid. 1823, p. 409.)
7. On the figure requisite to maintain the Equilibrium of a Homogeneous Fluid Mass that revolves upon an Axis. (Ibid. 1824, p. 85.)
8. On the Equilibrium of Fluids, and the Figure of a Homogeneous Planet in a Fluid State. (Ibid. 1831, p. 109.)
9. On the Theory of the Elliptic Transcendents. (Ibid. 1831, p. 349.)
10. On the Theory of the Perturbations of the Planets. (Ibid. 1832, p. 195.)
11. On the Development of the Disturbing Function, upon which depend the inequalities of the Motions of the Planets, caused by their mutual Attraction. (Ibid. 1833, p. 559.)
12. On the Equilibrium of a Mass of Homogeneous Fluid at liberty. (Ibid. 1834, p. 491.)
13. Of such Ellipsoids consisting of homogeneous matter as are capable of having the resultant of the attraction of the mass upon a particle in the surface, and a centrifugal force caused by revolving about one of the axes, made perpendicular to the surface. (Ibid. 1838, p. 57.)
14. On the Theory of the Astronomical Refractions. (Ibid. 1838, p. 169.)
15. On the Condition of Equilibrium of an Incompressible Fluid, the particles of which are acted upon by Accelerating Forces. (Ibid. 1839, p. 243.)
16. Note of Mr. Ivory, relating to the correcting of an error in a paper printed in the 'Philosophical Transactions' for 1838, pp. 57, &c. (Ibid. 1839, p. 265.)

Boyton House, near Heytesbury, and inherited the name of Bourke from his mother, who was the daughter of Viscount Mayo. He died at Kew on the 10th of January of the present year, having nearly completed his 81st year. His name appears among the original members of the Linnean Society, and for nearly fifty years he was one of its Vice-Presidents. He became a Fellow of the Royal Society in 1791, and consequently had belonged to it for more than half a century. He was an eminent botanist, and formed a very extensive herbarium, and was at all times anxious to give information to those attached to the same pursuit. He was the author of many papers in the Linnean Transactions, but his most considerable works were two separate publications. One on the genus Cinchona was given to the world in 1797. The other was a description of the genus Pinus,—a truly magnificent work, which originally came before the public in two vols. folio in the year 1803, to which a third vol. was added in 1834.

He married Catherine, daughter of Richard Bowater, Esq., whom he survived some years, and by whom he left no family. He did not furnish any papers to the Transactions of the Royal Society.

Sir Alexander Burnes is undoubtedly one of those whose death will be most lamented by a country that was proud of his eminent qualities, and grateful for his zealous services.

The name of Burnes was already distinguished in the northern portion of our island. It has received a new lustre from one well worthy of his descent from the same family as Scotland's celebrated poet. Sir Alexander was born at Montrose on the 16th of May, 1805. The same town had the honour of his education. He entered on his career of active service as a cadet of the Bombay army in the year 1821. At the early age of twenty he was appointed Persian interpreter to a force of 8000 men assembled under Colonel Napier for the invasion of Sinde. The following year he was appointed Deputy-Assistant-Quarter-Master-General.

He received, in 1827, the thanks of Government for an elaborate statistical report; and the following year, the Government showed itself equally satisfied with a valuable memoir of the eastern mouth of the Indus. This was succeeded by a valuable supplement.

In 1828, Lieut. Burnes applied for permission to visit the country between the Indus and Marwar; but though this plan was approved of by Sir John Malcolm and Sir Henry Pottinger, its execution was delayed. Burnes was appointed the same year Assistant-Quarter-Master-General, and received orders from the Court of Directors to complete a map of Cutch already commenced by him. Shortly after, he was appointed assistant to the political agent in Cutch, and published in the Transactions of the Royal Geographical Society an account of his survey of that country.

In 1830, he was sent with a present of horses from the King of England to Runjeet Singh. He visited Hydrabad, Lahore, Soodiana, and proceeded to Simla to receive further instructions from Lord W. Bentinck.

After travelling into Central Asia, he revisited Bombay in 1833;

thence he received orders to return home with his own despatches, and was received most cordially in England. His travels were now published, and met a most hearty welcome. They were immediately translated into the French and German languages, the best proof of their merit and importance being appreciated in other countries besides his own. He was warmly welcomed by the Royal Asiatic Society; and the French and English Geographical Societies bestowed on him their respective medals.

He enriched the national collection of the British Museum by presenting it with a collection of oriental coins.

After staying a year and a half in Europe, he returned to the East, and on his second arrival in India, he was sent on a mission to Hydrabad, which was entirely successful. The next, and unfortunately the last public duty in which he was employed was in a mission to Cabul, where those political events occurred which occasioned his falling a victim in his country's service at the early age of 36.

Such is a brief statement of the very active life of a man endowed by nature with an extraordinary variety of powers. Personally active and enterprising, he united to the qualities of the accomplished soldier and statesman those of the philologist and philosopher. What might we not have hoped from such a man, if Providence had seen right to prolong his days!

Sir Alexander was of a lively and playful disposition, and most amiable in private life. He was one of the best of sons and kindest of brothers.

George Fitzclarence, Earl of Munster, was born January 29, 1794. He entered the army at an early age, and served in the Peninsular war. He afterwards went to India, where he assiduously and successfully studied the Sanscrit, Arabic, and Persian languages. In 1818 he was entrusted with despatches announcing the conclusion of the Mahratta war; he seized the opportunity of acquiring and imparting additional knowledge, and travelled home by an overland route, publishing an account of his journey. He was created Earl of Munster soon after the accession of his late Majesty, William the Fourth.

Shortly after his return from India, he was elected a Vice-President of the Asiatic Society, and by his personal exertions procured much valuable information on oriental geography and statistics, and on the natural productions of India. He subsequently took a very active part in promoting the Oriental Translation Fund, and also the Society for the publication of Oriental Texts, and the Association formed for the purpose of increasing our knowledge of the countries south of Egypt. For the last fourteen years, he devoted great labour to the collection of materials for the compilation of a military history, and history of the civilization of the Mahommedan nations. Of this elaborate and important work, which was nearly completed, a long and interesting account is given in the Asiatic Journal. It is to be hoped that the friends of Lord Munster will not allow these labours to have been performed in vain.

It is needless for me to add what a severe loss his lordship's death must be to those who are interested in oriental pursuits, and indeed to his country itself, when we reflect on the large empire held by England in the eastern regions of the globe.

Lord Munster married Miss Wyndham in 1819, and has left a family to lament his death. He was elected President of the Asiatic Society only a short time before his decease.

John Yelloly, M.D., was born in 1773, at Alnwick in Northumberland, and received his early education at a school in that town. He chose medicine as his profession; and at the age of 20, went to Edinburgh, and after going through the usual course of study in its University, graduated there in 1796. Four years afterwards, he settled in London, and became a Licentiate of the College of Physicians. In 1806, he married Miss Tyssen, heiress to a considerable landed estate; and established himself in Finsbury Square. About this time, also, he was elected Physician to the Aldersgate-street Dispensary; and, in 1817, succeeded Dr. Cooke as Physician to the London Hospital. He became a Fellow of this Society in 1814.

Endowed by nature with great activity of mind, Dr. Yelloly applied himself with indefatigable industry to the acquisition and the extension of medical knowledge. His views were not confined to the narrow circle of his own individual advancement, but, embracing a wider range of utility, they extended not only to the improvement, but also to the general diffusion of science, and to whatever was calculated to raise the character and exalt the dignity of the profession to which he belonged. This liberal public spirit, indeed, was, throughout life, the main spring of his exertions; and one of its principal fruits was the formation, in conjunction with his friend Dr. Marcet, of the Medical and Chirurgical Society of London. The objects contemplated by such an institution were to establish a closer bond of union than had previously existed among the several branches of the medical profession; to collect a comprehensive medical library for their use; to read and discuss medical papers at the evening meetings; to publish a selection of these papers in the form of Transactions; to promote a free interchange of information, and to cultivate liberal and kindly feelings among the members. Many of the most eminent practitioners, both in Medicine and Surgery, were invited to join this new Society, which, from small beginnings, soon increased in numbers and in reputation, so as in the course of a few years to comprise a large portion of the professional rank and talent of the metropolis. It was to the active exertions and persevering zeal of its two founders that this Society was mainly indebted for its early success and its continued prosperity, amidst occasional difficulties with which it had to contend. Dr. Yelloly, in particular, devoted himself to its welfare with the attachment of a parent. At its commencement he officiated as Secretary, in conjunction with Mr. Charles Aikin; and for many years he was scarcely ever absent from its meetings, taking a lively interest in all its proceedings, and an active part in the discussions of the evening. To its Transactions

he contributed many valuable memoirs*. At a later period, about the year 1814, under the presidency of Sir Henry Halford, Dr. Yelloly, Dr. Marcet, and other influential members, conceiving that great advantages would result to the Society, and its permanence be better secured, by its being incorporated under a Royal Charter, took the proper measures for accomplishing this object. The necessary forms were gone through, and the grant was on the eve of being signed, when an unexpected opposition was suddenly raised by the College of Physicians, who finally prevailed on the Privy Council to refuse the prayer of the petitioners. Dr. Yelloly, however, lived to see the great change which has since taken place in the spirit of the times; for, in the year 1834, his favourite scheme was realised, all opposition had subsided, and the Society obtained at once from the Crown the Charter under which it is now constituted as the Royal Medical and Chirurgical Society of London.

Although Dr. Yelloly diligently availed himself of the extensive opportunities afforded by his public appointments, and had acquired universal respect and esteem by the suavity of his manners and the kindness of his disposition, it is remarkable that he nevertheless failed to obtain more than a very moderate share of private practice. In course of time his family had become very numerous, while his professional income was by no means increasing in an equal ratio; and prudential motives prevailing over his attachment to the metropolis, he at length determined to quit London, and establish himself at Carrow Abbey, in the immediate vicinity of Norwich. He resided there during many years, engaged in practice: he was soon elected one of the Physicians of the Norfolk and Norwich Hospital, and introduced into that establishment many useful reforms. It was during this period that he undertook the examination of the urinary calculi, of which the Hospital contained a large collection. He communicated to the Royal Society the result of his labours in a paper which was published in the volume of our Transactions for 1829†.

* These contributions were the following:—

1. A case of tumour in the brain, with remarks on the propagation of nervous influence. (November 29, 1808. Medico-Chirurgical Transactions, vol. i. p. 181.)

2. History of a case of Anæsthesia. (March 11, 1812. Ibid. vol. iii. p. 90.)

3. Observations on the vascular appearance in the human stomach, which is frequently mistaken for inflammation of that organ. (July 24, 1813. Ibid. vol. iv. p. 371.)

4. Particulars of a case in which a very large calculus was removed from the urethra of a female without operation; with examples of analogous cases. (June 20, 1815. Ibid. vol. vi. p. 574.)

5. Case of preternatural growth in the lining membrane covering the trunks of the vessels proceeding from the arch of the aorta. (July 8, 1823. Ibid. vol. xii. p. 565.)

6. Observations on the statement made by Dr. Douglass, of Cheselden's improved lateral operation of lithotomy; in a letter to Sir Astley Cooper, Bart., F.R.S. (April 14, 1829. Ibid. vol. xv. p. 339.)

7. Observations on vascular appearances of mucous and serous membranes, as indicative of inflammation. (Ibid. vol. xx. p. 1.)

† p. 55.

In this paper he gives an account of the structure and chemical composition of 330 calculi, which had either been purposely divided or accidentally broken in their extraction. The results are arranged in tables, exhibiting, in the order of their superposition from the centre, the consecutive deposits of which each calculus is composed. It appears from these tables, that not less than two-thirds of all urinary calculi consist of the lithates, or have those substances for their nuclei: whence Dr. Yelloly inferred the probability that a large proportion of them owe their existence to the previous formation of such a nucleus, and was led to suspect that carbonate of lime, although rarely found in a separate form in calculi, is not an unfrequent concomitant of phosphate of lime. With the assistance of Dr. Prout and Mr. Faraday, he ascertained the presence of carbonate of lime in some of the specimens which were not previously supposed to contain it; a result which was confirmed by the analysis of several calculi from the collection of the Hunterian Museum, and also from the Museum of Guy's Hospital.

He presented to the Society, two years afterwards, a sequel to this paper, recording, in a tabular form, the analysis of 335 additional specimens, which had, in the interval, been divided*. The most remarkable fact noticed in this memoir, is the presence of silex in a few specimens. Dr. Yelloly finds reason to believe that the average number of calculous disorders occurring in Scotland has been much underrated; that, on the other hand, the proneness to these complaints is very small in Ireland; and that, on the whole, a much larger proportion of calculous cases occurs in towns than in the country.

For some years before his death, Dr. Yelloly had relinquished practice, and resided at Woodton Hall, near Bungay; his attention being chiefly turned to agricultural pursuits. From thence he removed, about two years ago, to Cavendish Hall, in the neighbourhood of Clare, in Suffolk; where, in February last, his valuable life was suddenly terminated by an attack of apoplexy, while taking an airing in his carriage.

Lieutenant Wellstead, of the Indian Navy, was a distinguished traveller in the East. He was the author of a notice on the ruins of Berenice, of a journey into the interior of Oman, and of a journey to the ruins of Nahab el Hajar, published in the Transactions of the Royal Geographical Society. He died in the month of October last. He received a severe injury on the head while in India, which was the remote cause of his early and lamented death.

Mr. Hennell, the chemical operator at Apothecaries' Hall, lost his life by an extraordinary accident; he was mixing a large quantity of fulminating mercury for the service of the army in India, and being desirous that it should be of an uniform colour, the whole was placed in a large evaporating dish; as he was stirring it, an explosion of the whole took place, which was attended with his complete destruction, many parts of the body being thrown to a considerable di-

* Phil. Trans. for 1831, p. 415.

stance. He was an eminent chemist, and had furnished two papers to our Transactions*.

It is now, Gentlemen, time for me to perform the most agreeable part of the duty which falls to the lot of a President on your Anniversary—that of giving the Medals awarded by the Council. As we have not the pleasure of seeing here today Mr. MacCullagh, I shall beg Mr. Wheatstone, as his friend, to transmit his Medal to that gentleman.

MR. WHEATSTONE.

It gives me great satisfaction to be the organ of the Council of the Royal Society in bestowing on your friend Mr. MacCullagh the Copley Medal. It is needless for me to dilate on the profound mathematical skill and exemplary diligence with which he has explained the laws of the undulatory theory of light. Philosophers more able than myself to appreciate their merits, have given their testimony to the great value of his discoveries, and to the elegant means that he has employed. It is the sincere wish of us all, that these labours may be followed by others as important to science and as honourable to the University of Dublin ; an University that numbers Mr. MacCullagh among the most eminent of her sons.

The Council have awarded the Copley Medal for the present year to Professor MacCullagh, for his researches connected with the wave-theory of light, contained in the Transactions of the Royal Irish Academy. The grounds on which they have made this award are the following. One of the most important steps made in the physical theory of light, since it was first promulgated by Huygens, is, undoubtedly, Fresnel's discovery of the laws of refraction by crystallized media, embodied in his 'Mémoire sur la double réfraction.' The object proposed by Professor MacCullagh, in his first paper †, was to simplify and to develope that theory. He has shown in this paper, that the elastic force of the luminiferous æther may be represented, in magnitude and direction, by means of an ellipsoid, whose semiaxes are the three principal refractive indices of the medium ; and he has thence deduced, in a geometrical form, the leading results of Fresnel's theory. This ellipsoid is closely related to the generating ellipsoid of Fresnel ; and by the aid of these relations, Professor MacCullagh has demonstrated, in a very simple manner, the truth of Fresnel's construction of the wave-surface, the demonstration of which had been left imperfect by its author.

In Mr. MacCullagh's next paper, entitled "Geometrical propositions applied to the Wave-theory of Light,‡" he has examined the properties of a surface, which he calls the *surface of indices*, and which had presented itself likewise in the researches of M. Cauchy and Sir William Hamilton ; and he has shown that it affords a general and exact construction for the *interval of retardation* of the two rays in

* Lord Vivian, the Earl of Macclesfield, and Mr. Gage Rokewode, with other deceased Fellows of the Society, were also noticed in the President's Address.—EDIT.

† Transactions of the Royal Irish Academy, vol. xvi.

‡ Ibid. vol. xvii.

their passage through a double-refracting crystal; and thus that the forms of the rings, or isochromatic curves, which had previously been deduced only by approximate methods, may be determined generally.

The next paper of Professor MacCullagh is that "On the Double Refraction of Quartz*;" a subject which had engaged the attention, successively, of Biot, Fresnel, and Airy. The first of these writers had determined experimentally the laws of rotatory polarization, which take place when a ray is transmitted along the axis of rock-crystal; and the second had shown that these laws were explained by the interference of *two circularly polarized* rays, which are transmitted *along the axis* with different velocities. The next step in this curious subject was made by Mr. Airy, who examined the peculiar phenomena of refraction by quartz in *other directions*, and showed that they were accounted for by the supposition of two *elliptically polarized* rays, the ratio of the axes of these elliptical vibrations varying with the inclination of the rays to the axis of the crystal. Lastly, Professor MacCullagh has shown that both the circular polarization of the rays in the axis, and the elliptical polarization of the rays inclined to it, may be explained by a certain assumed form of the differential equations of vibratory movement, which not only links together the two classes of phenomena, but also affords a mathematical expression for their laws. The general theory, to be alluded to presently, has enabled him to explain the origin of these assumed forms of the differential equations.

The theory of reflexion at the surfaces of uncrystallized media had been given by Fresnel, although apparently on erroneous principles. The more complex case of reflexion at the surfaces of crystals was left by him to his successors; and the discovery was made independently, and nearly at the same time, by Professor MacCullagh† and M. Newmann of Königsberg. The discovery is not only important in itself, as bringing within the domain of the wave-theory a large class of hitherto unexplained phenomena, but perhaps still more on account of the physical principles upon which it is based, and the constitution of the luminiferous æther which it renders probable. Thus, it is assumed in this theory, in opposition to the hypothesis of Fresnel, that the *vibrations are parallel to the plane of polarization*, and that the *density of the æther is the same in all media*. These, together with the law of the *vis viva*, and the beautiful principle of the *equivalence of vibrations* (but half perceived by Fresnel), form the foundation of the theory of crystalline reflexion, and derive the highest probability from its accordance with phenomena. The results of the theory are embodied in geometrical constructions of great elegance, which determine generally the plane of polarization of the reflected ray, and the amplitudes of the reflected and refracted vibrations.

Hitherto the laws of reflexion at the separating surface of two me-

* Transactions of the Royal Irish Academy, vol. xvii.

† "On the laws of crystalline reflexion and refraction." Transactions of the Royal Irish Academy, vol. xviii. This memoir has been honoured by the Medal of the Royal Irish Academy. [Phil. Mag. S. 3. vol. xi. p. 134.]

dia were apparently unconnected with those which govern the propagation of light in the same medium. It remained to connect these laws as parts of one and the same system, and to trace the hypothetical principles upon which each theory was based, up to some higher mechanical principle. This crowning point of the theory has been attained by Professor MacCullagh*. Employing the general processes of analytical mechanics, as laid down by Lagrange†, and limiting the general theorems solely by the conditions that the density of the æther is *constant*, and that the vibrations are *transversal*, he has succeeded in deducing, as parts of one and the same general theory, not only the laws of propagation in the same medium, previously discovered by Fresnel, but also the laws of reflexion which take place at the bounding surface of any two media, already discovered by himself and M. Newmann. The same theory has likewise led to the *demonstration* of those physical principles, which had been *assumed* in the former paper. It has shown that the *vis viva* is necessarily preserved, in the passage of light from one medium into another; that the resultants of the vibrations are the *same in the two media*; and finally, that the vibrations themselves are *parallel to the plane of polarization*.

This seems to be the most advanced point to which the physical theory of light, in its present form, is capable of being pushed; and it is only by the addition of *new physical principles*, and further insight into the constitution of the luminiferous medium, that any ulterior progress can be expected.

MR. FOX TALBOT.

The many important discoveries made by you in Photography, discoveries to which I have adverted when addressing the Society on another occasion, discoveries which seem, with those of an analogous nature made by a Neipsce and a Daguerre, to open to us the vista of discoveries still more vast and curious, undoubtedly well entitle you to the honour of the Rumford Medal at our hands. Your papers, indeed, have been so great an ornament to our volumes, that we can never sufficiently express our thanks to you for them. I trust that you will not desert so promising a line of inquiry, and that our Transactions may receive from you still greater acquisitions of knowledge in the path which is traced by light itself‡.

MR. BOWMAN.

It must be always satisfactory for a President of the Royal Society to present to one of your profession a Royal Medal for labours which have as their instruments, the assiduous application of the noblest faculties of reason—as their immediate purpose, the knowledge of the sublime truths contained in the wonderful adaptations of the organs of

* Proceedings of the Royal Irish Academy for December 1839. The complete paper has not yet been published. [Phil. Mag. S. 3. vol. xxi. p. 228.]

† Mr. Green appears to have been the first to apply these methods to the dynamics of light, in a paper on the laws of reflexion and refraction at the surfaces of uncrystallized media, published in the Cambridge Transactions. He has failed, however, in assigning the form of the principal function, and has consequently been led to erroneous results.

‡ See Phil. Mag. S. 3. vol. xix. p. 164.

created beings—and as their ultimate end, the cure of disease, the alleviation of agony, and the prolongation of human life. Gentlemen of your own valuable profession have given their testimony to the importance of your discoveries, and the Council feels pleasure in rewarding your zeal and talents*. To you, and all who, like you, are employed in these noble pursuits, all here will say with me, may God prosper your labours to His glory and to the happiness of His creatures.

MR. DANIELL.

The continued intercourse that I have had with you in the Council of the Royal Society increases the pleasure which I experience in giving into your hands this Medal. Electrical Chemistry, at all times of great importance as giving us an insight into the most recondite laws of nature, has now acquired additional interest by the practical purposes to which a Wheatstone, a Spencer, a Jacobi, and others have applied it. Its connection with magnetism seems to promise still greater discoveries than those that have already immortalised a Davy and a Faraday. You have pursued this difficult branch of Chemistry with signal success, and the Council have approved of the recommendation of the Chemical Committee, that one of the Royal Medals should be conferred on you for the valuable papers which you have contributed to our Transactions. I trust that our future volumes may be still more enriched by the result of your scientific labours†.

The following Gentlemen were duly elected Officers and Council for the ensuing year, viz:—

President.—The Marquis of Northampton. *Treasurer.*—Sir John William Lubbock, Bart., M.A. *Secretaries.*—Peter Mark Roget, M.D., Samuel Hunter Christie, Esq., M.A. *Foreign Secretary.*—John Frederic Daniell, Esq. *Other Members of the Council.*—George Biddell Airy, Esq., M.A., A.R.; Francis Baily, Esq.; Martin Barry, M.D.; Henry James Brooke, Esq.; Robert Brown, Esq., D.C.L.; Rev. James Cumming, M.A.; John Thomas Graves, Esq., M.A.; Sir William J. Hooker, K.H., LL.D.; Robert Lee, M.D.; Gideon A. Mantell, Esq., LL.D.; William Hallows Miller, Esq., M.A.; William H. Pepys, Esq.; George Rennie, Esq.; The Earl of Rosse; William Henry Fox Talbot, Esq.; Charles Wheatstone, Esq.

XXII. *Intelligence and Miscellaneous Articles.*

ON THE FORCE OF AQUEOUS VAPOUR, IN REPLY TO MR. MOYLE. BY J. APJOHN, M.D., M.R.I.A.

To the Conductors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN reply to Mr. Moyle's inquiry contained in page 73 of the Philosophical Magazine for last month, I beg to say that my method of reducing to 32° the pressures mentioned in a short communication of mine to the Royal Irish Academy, "On the force of aqueous vapour within the range of atmospheric temperatures," which you

* See Phil. Mag. vol. xx. p. 509. † Ibid. vol. xxi. p. 54.

have transferred to the November Number of your Journal, consisted in first bringing the observed pressures to what they would be at the *neutral point* of temperature, applying next the correction for *capacity*, then bringing the resulting barometric heights to what they would be at 32°, and lastly adding the corrections for capillarity. There is, I need not say, nothing peculiar in this process. Not having by me the portable barometer (one by Newman) which I used in my experiments, I cannot exactly state the neutral points of pressure and temperature, and the corrections for *capacity* and *capillarity* peculiar to it, and which are, as usual, engraved upon its mounting. I may mention, however, that the coefficient of mercurial expansion for one degree Fahrenheit which I have employed in the reductions is ·0001; the number very nearly which results from the well-known experiments of Dulong and Petit. I apprehend that Mr. Moyle has been led into error by supposing me to have used a syphon barometer with moveable scales, an instrument whose indications require to be corrected for temperature alone.

I am, Gentlemen, your obedient Servant,

28 S. Baggott-Street, Jan. 16, 1843. JAMES APJOHN.

ON THE EXTRAORDINARY DEPRESSION OF THE BAROMETER ON JANUARY 13TH, 1843. BY H. H. WATSON, ESQ.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

The very extraordinary depression of the barometer which occurred yesterday, will doubtless have been noted by many of your readers; and probably the subjoined account of the observations made by me, at this town, on the height of the mercurial column, will not be unacceptable for publication in your Journal; as, by comparison with the notes made by distant observers, they may assist in showing how far the depression has been general.

13th Jan. 1843.	Height of the Mercurial column.	13th Jan. 1843.	Height of the Mercurial column.
9 A.M.	27·81 inch.	Half-past 3 P.M.	27·81 in.
10 A.M.	27·77 ...	4 P.M.	27·85 ...
Half-past 10 A.M.	27·75 ...	Half-past 4 P.M.	27·87 ...
11 A.M.	27·73 ...	5 P.M.	27·87 ...
Half-past 11 A.M.	27·72 ...	Half-past 5 P.M.	27·87 ...
12	27·72 ...	6 P.M.	27·88 ...
Half-past 12 P.M.	27·72 ...	7 P.M.	27·94 ...
1 P.M.	27·73 ...	8 P.M.	27·97 ...
Half-past 1 P.M.	27·74 ...	9 P.M.	28·01 ...
2 P.M.	27·76 ...	10 P.M.	28·03 ...
Half-past 2 P.M.	27·77 ...	11 P.M.	28·04 ...
3 P.M.	27·79 ...		

The greatest depression was from half-past 11 A.M. to half-past 12; the height of the mercury then being 27·72 inches. The mean annual height of the barometer at this town, as obtained from my observations made morning, noon, and night during twelve years, com-

mencing January 1831, is 29·564 inches. With the exception of yesterday, the lowest height of the barometer during the time which has elapsed since I commenced keeping a register, was 28·04 inches: it occurred at 6 P.M. on the 13th of November 1840.

The weather in the early part of yesterday, and till evening, was similar to what it had been for several days previously; a little snow, hail and rain falling at intervals. The wind was rather south of west, but not strong, till about 7 P.M., when it began to blow strongly, and during the night and till daybreak this morning was very boisterous; it then began to abate, and by 9 A.M. was not unusually strong.

The temperature here yesterday ranged from 35° to 36°; today it has ranged from 34° to 30°: at half-past 10 on the night of the 12th instant it was 27°, and the height of the barometer at that time was 28·70 inches. Today the height of the barometer has been at 8 A.M. 28·58 inches; at half-past 12 P.M. 28·54 inches, and at half-past 10 at night 28·57 inches; the weather similar to that of the several previous days.

It will be found by reference to the observations of Mr. Luke Howard, page 69, vol. iii. of his work, entitled "The Climate of London," that a depression of the barometer, similar to that of yesterday, occurred in December 1821.

I remain, Gentlemen, yours most respectfully,

Bolton-le-Moors, Jan. 14, 1843. HENRY HOUGH WATSON.

METEOROLOGICAL OBSERVATIONS FOR DECEMBER 1842.

Chiswick.—Dec. 1. Slight rain: overcast. 2. Densely clouded: clear and fine. 3. Foggy. 4. Foggy: overcast. 5. Light haze: very fine: foggy. 6—9. Foggy. 10. Overcast. 11. Foggy: clear and fine. 12. Rain: overcast and mild. 13. Very fine: overcast. 14, 15. Exceedingly fine. 16. Very fine: densely overcast. 17. Very fine. 18, 19. Foggy: clear and fine. 20, 21. Hazy. 22. Very fine. 23. Rain. 24. Very fine. 25. Clear: overcast and fine: stormy at night. 26. Cloudy and windy. 27. Rain: cloudy and damp: frosty. 28. Frosty: clear and fine. 29. Densely clouded. 30. Cloudy and very mild. 31. Very fine.—Mean temperature of the month 4°·12 above the average.

Boston.—Dec. 1—3. Cloudy. 4. Foggy. 5. Cloudy. 6. Foggy. 7. Cloudy. 8, 9. Foggy. 10. Foggy: rain early A.M. 11. Cloudy. 12. Rain: rain early A.M. 13. Cloudy. 14, 15. Fine. 16. Cloudy: rain P.M. 17—19. Fine. 20. Cloudy. 21, 22. Fine. 23. Cloudy. 24. Fine. 25. Fine: rain P.M. 26. Windy: rain P.M. 27. Cloudy: rain early A.M. 28. Fine. 29. Windy. 30. Fine. 31. Windy: stormy P.M.

Sandwick Manse, Orkney.—Dec. 1. Rain: cloudy. 2. Showers: cloudy. 3. Clear: showers. 4. Cloudy: drizzle. 5, 6. Bright: cloudy. 7. Cloudy. 8. Drizzle. 9. Fog. 10. Fog: cloudy. 11. Cloudy. 12. Rain: cloudy. 13, 14. Cloudy. 15. Bright: cloudy. 16. Bright. 17, 18. Showers. 19. Showers: clear. 20. Showers: cloudy. 21. Cloudy: drizzle. 22. Showers. 23. Showers: snow. 24. Showers. 25. Rain. 26, 27. Hail-showers. 28, 29. Cloudy. 30. Rain: drizzle. 31. Showers: frost.

Applegarth Manse, Dumfries-shire.—Dec. 1, 2. Rain and wind. 3. Fine and fair. 4. Rain A.M.: cleared. 5, 6. Rain P.M. 7. Fair and fine. 8. Fair A.M.: drizzly P.M. 9. Fair but cloudy. 10. Drizzly. 11. Fair: overcast P.M. 12. Wet all day. 13. Storm: wind: rain: flood. 14. Rain P.M. 15, 16. Storm: wind: rain P.M. 17. Fair. 18—23. Showers P.M. 24. Hoar-frost A.M. 25. Very wet all day. 26. Very wet A.M. 27. Slight shower: frost P.M. 28. Frost A.M.: rain P.M. 29. Rain, but mild. 30. Rain and high wind. 31. Rain: cleared P.M.

The high temperature of December is remarkable, being nearly 10° higher than the mean of the last twenty years, and 7° higher than Dec. 1841.

Meteorological Observations made at the Apartments of the Royal Society, LONDON, *by the Assistant Secretary*, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *by the* Rev. W. Dunbar, *at Applegarth Manse*, DUMFRIES-SHIRE; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Barometer.

Days of Month. 1842. Dec.	London: Roy. Soc. 9 a.m.	Chiswick. Max.	Chiswick. Min.	Boston. 8½ a.m.	Dumfries-shire. 9 a.m.	Dumfries-shire. 9 p.m.	Orkney, Sandwick. 9½ a.m.	Orkney, Sandwick. 8½ p.m.
1.	30·062	30·017	29·997	29·56	29·62	29·50	29·60	29·38
● 2.	30·110	30·079	30·028	29·48	29·58	29·73	29·50	29·62
3.	30·250	30·382	30·200	29·63	29·94	30·14	29·73	30·00
4.	30·454	30·406	30·341	30·03	30·12	30·04	29·76	29·78
5.	30·344	30·297	30·212	29·85	29·98	30·01	29·90	30·12
6.	30·264	30·223	30·210	29·73	30·03	30·03	30·18	30·06
7.	30·350	30·333	30·309	29·86	30·06	30·11	29·95	29·91
8.	30·442	30·404	30·389	29·96	30·22	30·28	30·11	30·32
☽ 9.	30·424	30·397	30·282	30·00	30·30	30·22	30·39	30·38
10.	30·256	30·265	30·165	29·83	30·07	30·00	30·18	30·03
11.	30·026	30·017	29·851	29·66	29·82	29·61	29·93	29·80
12.	29·820	29·942	29·802	29·34	29·38	29·53	29·42	29·41
13.	30·076	30·017	29·999	29·44	29·58	29·60	29·48	29·58
14.	30·144	30·088	30·073	29·56	29·81	29·72	29·74	29·68
15.	30·164	30·106	30·091	29·55	29·71	29·70	29·49	29·67
16.	30·090	30·038	29·952	29·50	29·72	29·51	29·76	29·43
○ 17.	30·002	30·170	29·882	29·37	29·59	29·80	29·34	29·56
18.	30·314	30·402	30·281	29·75	29·90	30·10	29·73	29·92
19.	30·518	30·485	30·448	29·99	30·26	30·12	30·09	29·89
20.	30·430	30·397	30·312	29·87	30·12	30·11	30·00	29·78
21.	30·328	30·268	30·234	29·78	30·10	30·00	29·97	29·80
22.	30·194	30·164	29·853	29·59	29·73	29·42	29·36	29·09
23.	29·636	29·614	29·551	29·14	29·27	29·20	28·96	29·00
☾ 24.	29·696	29·794	29·651	29·15	29·40	29·50	29·26	29·43
25.	29·848	29·815	29·659	29·47	29·35	29·15	28·83	28·67
26.	29·566	29·548	29·141	29·07	28·96	29·01	28·67	28·86
27.	29·364	29·661	29·321	28·90	29·11	29·42	29·12	29·32
28.	30·030	30·144	29·929	29·53	29·73	29·75	29·61	29·65
29.	30·152	30·174	30·112	29·62	29·61	29·83	29·14	29·80
30.	30·222	30·215	30·161	29·64	29·70	29·75	29·38	29·44
● 31.	30·144	30·178	30·046	29·45	29·64	29·95	29·45	29·99
Mean.	30·120	30·130	30·038	29·59	29·755	29·769	29·613	29·657

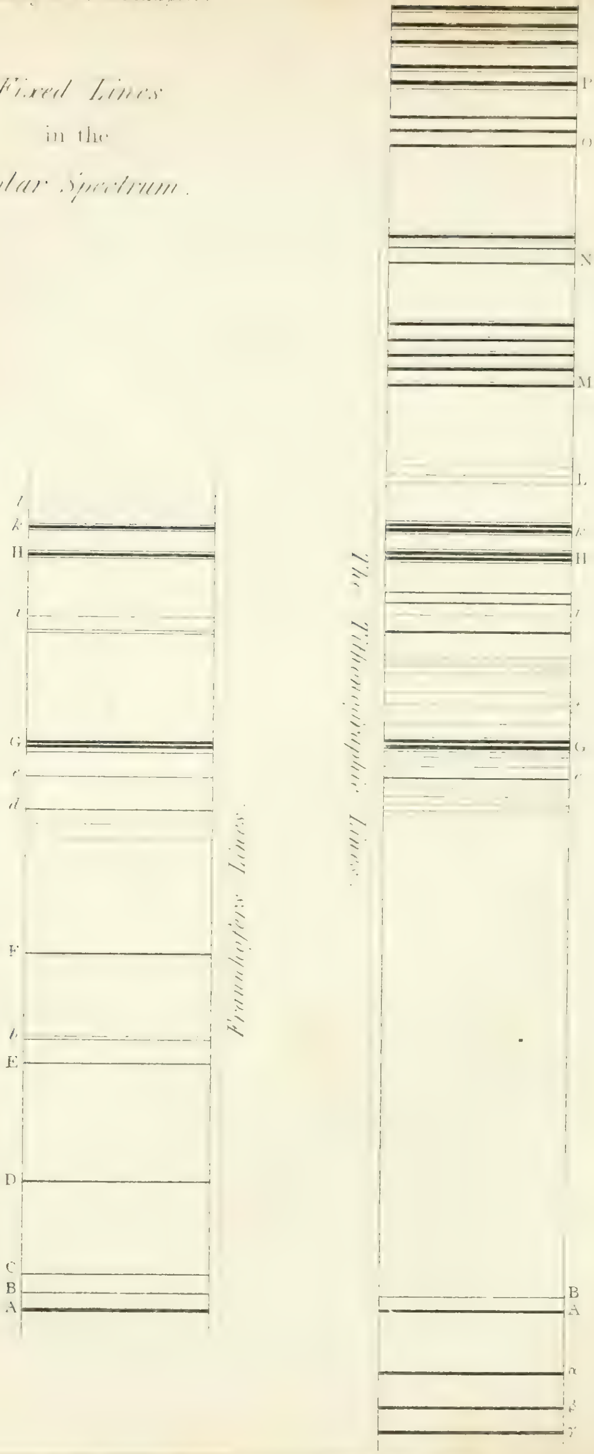
Thermometer.

Days of Month. 1842. Dec.	London: R.S. Fahr. 9 a.m.	London: R.S. Self-reg. Mx.	London: R.S. Self-reg. Mn.	Chiswick. Max.	Chiswick. Min.	Boston. 8½ a.m.	Dumf.-shire. Max.	Dumf.-shire. Min.	Orkney, Sandwick. 9½ a.m.	Orkney, Sandwick. 8½ p.m.
1.	49·7	51·0	43·3	54	52	44	54	39	44	50
● 2.	52·0	55·0	49·8	55	32	55	54	49½	50	46
3.	46·7	56·3	45·5	53	42	43·5	51	47	46	46
4.	47·7	54·7	44·4	52	39	36·5	49½	43	50	49
5.	44·7	52·6	44·6	52	39	45	49	47	47	44½
6.	41·3	50·6	40·8	40	36	40	51	46	41½	43½
7.	39·5	42·8	37·8	39	33	42	47½	44½	48	48
8.	37·3	41·6	36·0	37	31	35	49½	45	50	48
☽ 9.	41·7	43·2	36·6	43	36	38·5	48	41½	46	44
10.	37·8	45·0	37·5	39	38	43	47	43½	44½	45
11.	38·7	40·8	37·3	43	38	38	46½	35½	43	44½
12.	50·3	51·5	38·8	57	50	47	53	41	46	49
13.	51·6	56·5	38·8	61	42	44	55	50	48	48
14.	49·8	57·5	49·2	56	36	47	55	48	48	48
15.	48·3	55·0	46·8	56	38	45	56½	51	54	50
16.	50·0	54·6	47·7	55	48	48	55	46½	44½	46
○ 17.	50·5	55·6	50·2	51	31	49	50	40	43	44½
18.	39·4	52·6	39·3	47	35	40·5	47	40	43½	42
19.	39·3	46·0	39·0	45	35	40·5	47	36½	46½	46
20.	45·7	46·4	39·0	52	46	47	49	44	48	49
21.	51·3	51·4	39·0	55	48	51	53	44	48	46
22.	50·2	56·3	49·0	54	45	47	52½	45	46	42
23.	47·4	53·8	47·6	46	31	46	48	35	37	34
☾ 24.	36·7	47·4	36·2	50	25	37	48	35	40	40
25.	36·7	42·4	35·0	50	48	34	51	31½	47	48½
26.	48·8	50·3	37·3	51	41	51	50	46	40½	41
27.	43·7	50·6	44·0	47	25	43	47	31	40	40
28.	35·0	47·0	34·7	43	27	35	45½	30½	41	39½
29.	47·0	48·2	35·4	52	46	48	51	39	46	43½
30.	52·3	53·3	47·2	55	46	53·5	54	44	45½	45½
● 31.	51·3	55·4	50·6	54	30	54	51	50	41	34
Mean.	45·2	50·5	41·9	49·80	38·35	44·1	50·4	42·2	45·27	44·67

Wind. / Rain. / Dew-point.

Days of Month. 1842. Dec.	Wind. London: R.S. 9 a.m.	Wind. Chiswick. 1 p.m.	Wind. Boston.	Wind. Dumfries-shire.	Wind. Orkney, Sandwick.	Rain. London: R.S. 9 a.m.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Rain. Orkney, Sandwick.	Dew-point. Lond. R.S. 9 a.m.
1.	se.	s.	calm	sse.	se.	……	·01	……	……	·08	46
● 2.	sse.	s.	sw.	s.	w.	……	……	……	……	·33	49
3.	s.	sw.	calm	wsw.	w.	……	……	……	……	……	46
4.	w.	s.	calm	s.	s.	……	……	……	……	·05	47
5.	ssw.	sw.	calm	s.	w.	……	……	……	……	·17	44
6.	sse.	s.	calm	s.	s.	……	……	……	……	……	43
7.	w.	s.	calm	s.	s.	……	·01	……	……	……	38
8.	w.	sw.	calm	w.	w.	……	……	……	0·87	……	39
☽ 9.	w.	se.	calm	ese.	se.	……	……	……	……	·08	40
10.	se.	s.	calm	ese.	se.	……	……	·04	……	……	38
11.	e.	se.	calm	ese.	se.	……	·32	……	……	……	38
12.	se.	s.	sw.	s.	se.	·269	·01	·08	……	……	43
13.	se.	s.	calm	s.	s.	……	……	……	……	·37	48
14.	s.	s.	calm	e	s.	……	……	……	……	·06	48
15.	s.	s.	calm	s.	s.	……	……	……	……	·02	46
16.	s.	sw.	calm	s.	s.	……	·06	……	0·88	……	48
○ 17.	ssw.	w.	calm	sw.	w.	·050	……	·06	……	·12	48
18.	s.	w.	calm	sw.	w.	……	……	……	……	·16	42
19.	w.	w.	w.	sw.	sw.	……	·01	……	……	·20	40
20.	s.	sw.	w.	ssw.	sw.	……	·01	……	……	·06	43
21.	w.	sw.	w.	s.	w.	……	·01	……	……	·02	46
22.	s.	w.	w.	s.	sw.	……	……	……	……	·10	46
23.	s.	sw.	w.	sw.	s.	·075	·09	……	0·56	·18	48
☾ 24.	s.	sw.	w.	w.	nw.	·097	……	……	……	·20	39
25.	s.	sw.	calm	sw.	sw.	……	……	……	……	·36	37
26.	s. var.	sw.	sw.	w.	w.	·022	·20	·07	……	·31	45
27.	s.	w.	calm	var.	sw.	·161	·01	·63	……	·19	43
28.	w.	w.	calm	e.	wnw.	……	……	……	……	·11	36
29.	sw.	w.	w.	sw&w	w.	……	……	……	……	·11	42
30.	s. var.	w.	calm	sw.	sw.	……	……	……	0·60	·59	45
● 31.	s. var.	w.	w.	sw.	n.	……	·02	……	……	·57	47
Mean.						Sum. 0·674	0·76	0·88	2·91	4·44	Mean. 44

Fixed Lines
in the
Solar Spectrum.



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THE

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[THIRD SERIES.]

MARCH 1843.

XXIII. *On the rapid Detithonizing Power of certain Gases and Vapours, and on an instantaneous means of producing Spectral appearances. By* John W. Draper, *M.D., Professor of Chemistry in the University of New York**.

FOR some time after I was acquainted with the phænomena mentioned in the last paper communicated in this Journal,† and there referred to radiation, I was led to attribute them to a peculiar property which certain gases and vapours possess, of which I propose now to give a detailed description.

This property is a power of effecting a very rapid detithonization of surfaces that have been *powerfully* tithonized.

It affords the means of *instantly* procuring spectral appearances of external forms.

Referring now, in the first place, to the analogies of caloric: a body which has been warmed cools down to a temperature that is in equilibrium with that of objects around in several different ways, by radiation, by currents in the air, and often by direct conduction, each of these tending to produce the same result.

A sensitive surface, which has been disturbed by exposure to the daylight or lamplight, has the quality of restoring itself to its primitive condition when kept in the dark. Daguerre noticed this in the case of certain resinous bodies; other experimenters have likewise proved that it takes place with some varieties of the ordinary photogenic preparations. I have found that it holds in the coloured films on the surface of silver.

Much of this effect is due, as I have endeavoured in the paper above quoted to show, to a direct escape of dark rays by a process analogous to radiation; but much also is due to a hitherto unknown power, possessed by electro-negative gases

* Communicated by the Author.

† S. 3. vol. xxi. p. 453: see also p. 131 of our last Number.—Ed.

and vapours, which tends to bring about the same results. So powerfully indeed does this cause operate, that, as I have said, for a length of time I attributed all the phænomena to it.

I proceed now to describe some simple experiments which will bring this matter clearly before the reader.

Take a bromo-iodized silver plate, expose it to the light of the sky or lamplight for a length of time sufficient to brown it sensibly and uniformly all over. In this state, if it were placed in the vapour of mercury, it would solarize or blacken in every part. But, before mercurializing, treat it as follows. Lay upon it a fragment of glass, a piece of metal, or any other object; immerse it for a second or two in a box containing the vapour of iodine; withdraw it, remove the little object, and mercurialize forthwith: and now you will find a perfectly formed, *black*, spectral impression of the object, whatever it was, powerfully brought out by the mercury vapour; but on all those parts to which the iodine vapour has had access, the mercury will not adhere, but the phænomenon will take effect as though the plate had never been exposed to the light, except on those portions on which the object, whose spectral image appears, was laid.

From this it would seem that the vapour of iodine has the quality of detithonizing a surface that has been changed by light.

The same process may be conducted so as to give a still more striking result.

Employing a prepared bromo-iodized plate as before, expose it to any uniform source of light for such a length of time that if it were mercurialized it would whiten uniformly and exhibit the aspect of an ordinary white Daguerreotype. Treat it as before, by placing on it any object, pass it into the vapour of iodine,—remove the object, and mercurialize: and now a spectral appearance of that object, of a dense *white* aspect, will emerge, the remainder of the plate being quite black and in the condition of the shadows of a Daguerreotype, that is, as though it had never been exposed to the light.

In order to obtain a clear idea of what passes under the foregoing circumstances, I made the following trial.

Upon a plate prepared and deeply tithonized, as has been said, I laid a double convex lens of about two inches focus, and exposing the plate with the lens upon it to the vapour of iodine, and then removing the lens, I mercurialized. A deep blue spectral image emerged, of less diameter than the lens, but like it of a circular form, its circumference being marked by as *sharp* a line as if it had been drawn by a pair of compasses. Indeed, it looked as distinct and as *sharp* as if a blue wafer had been laid on the plate.

In several successive trials I found that the magnitude of this spectre diminished as the time of exposure to the vapour had been prolonged.

Next, I repeated the same trial, using the plate and lens as just described; but immersing the plate in the vapour of bromine instead of that of iodine,—a still more remarkable image emerged on mercurializing. This image, like the former, was circular and black, but all round it for a certain space there was an annulus of narrow dimensions of pure unmercurialized silver, the deep black of which contrasted strikingly with the blue black of the spectre, and its outer circumference was marked by a faint whitening of the plate,—faint, but as sharp as it is possible to conceive.

In a third trial things were conducted as before, except that now chlorine, diluted with atmospheric air, was used; the spectre again came out, and did not differ in any observable manner from that produced by iodine.

In a fourth trial the vapour of nitrous acid was used as a detithonizer. In this case the edges of the spectre commonly had a gradually shading outline, and only in one instance did I find that sharpness of termination exhibited in the other cases.

We therefore perceive that iodine, bromine, chlorine, and nitrous acid can detithonize a surface on which light has fallen: *they can undo what the tithonic rays have done.*

In repeating these experiments, as for example the one by iodine, if the common iodine-box be used to effect the detithonization, two or three seconds of time is all that is required. A longer period is demanded when the vapour is very weak, but when strong the effect is *almost instantaneous.*

This detithonization and production of spectral images can therefore be accomplished in an incredibly short space of time.

I made trials with other substances, such as hydrogen gas and the vapour of liquid muriatic acid. The former to a certain extent, though not near so powerfully as the electro-negative bodies mentioned, could produce the change in question; the latter seemed to be without any perceptible action.

To the list, with the other electro-negative substances, I believe oxygen ought to be added; for, on repeating the same experiment and raising the temperature of the plate in atmospheric air so as to maintain the tithonized surface at about 200° Fahr. for several minutes, a certain effect which in an imperfect way resembled those already described was exhibited. Oxygen, therefore, diluted as in atmospheric air, at 200° Fahr., may be regarded as possessing to a small extent the property in question.

Without multiplying the description of these experiments further,—for the ingenuity of any one who repeats them will suggest many modifications which may give rise to striking results,—I will in conclusion give the reasons which have led me to suppose that in all these phænomena two different principles are engaged,—vapour action, and radiation.

I have stated that the Electro-negative bodies possess this detithonizing quality in a very marked manner. I do not wish it to be understood, however, that there is any relation of antagonization between that particular class of substances and the tithonic rays. It appears to me that their peculiar quality, in the circumstances described, may be traced to the fact, that silver, an electro-positive substance, happens to afford the sensitive surface. I have however prepared a paper which takes up the consideration of the conditions and theory involved, and will not at present anticipate what has to be offered when that paper shall be published.

The action, then, which these different gases and vapours exhibit, is so intense as to mask the feebler effect of radiation. Thus it takes several hours' exposure in the dark, and after a long subsequent process of mercurialization, to prove the true radiant effect,—a slow detithonization, which could be brought about by vapour action *in an instant*. But whoever has seen the symmetrical or rather geometrical lines that are left, when the slower process is followed, must be struck with the persuasion that the phænomenon he witnesses is obeying geometrical laws, and is not due to the irregular action of a dilute and varying current of vapour.

Thus, on repeating *carefully* the experiment cited at the close of my last paper, in which a lens is laid on a tithonized surface and left in the dark, I found that after the mercurial process was completed, the plate exhibited a dark central spot surrounded by a white annulus. On drawing upon paper a section of the lens and the sensitive surface, I found that a line drawn from the extreme edge of the white annulus to the edge of the lens was *a tangent* to the lens at that point; that a line drawn from the extreme edge of the central dark spot would, after reflexion by the convex surface of the lens, be found precisely on the edge of the white annulus; the edge of the annulus and the edge of the spot thus having a true catoptrical relation to the curvature of the lens.

Now, although in laboratories such as that in which my experiments are conducted, the vapours of these different electro-negative bodies are unquestionably present, and may produce a part of the phænomenon witnessed, yet inasmuch as that phænomenon follows laws that are apparently of a

strict geometrical kind, and to those floating vapours we could hardly assign anything like symmetrical results,—guided, also, by the analogy of cooling bodies, which lose part of their heat through radiation, and part through the current action of the air, and part through the conducting power of their supports, I have been led to take the view of the phænomena in question which I have set forth.

University, New York, Dec. 5, 1842.

XXIV. *On the Theory of the Gaseous Voltaic Battery.* By C. F. Schœnbein, *Professor of Chemistry in the University of Bâsle.*

To R. Taylor, Esq.

My dear Sir,

It was with no small degree of interest that I perused the other day Mr. Grove's communication [Phil. Mag. S. 3. vol. xxi. p. 417], containing a description both of a gaseous voltaic battery, and of some experiments made with that arrangement. The results obtained by that distinguished philosopher are, indeed, such as will certainly draw upon them the attention of all scientific men who occupy themselves with voltaic researches. Having myself made some investigations concerning a similar subject, and ascertained a series of facts which, in my opinion, are closely connected with Professor Grove's beautiful experiments,—and a good deal of scientific interest being attached to the matter in question—I take the liberty to direct your attention to the contents of a paper of mine which was published first in the Transactions of the Swiss Association, 1841, and afterwards in Poggendorff's *Annalen*, 1842, No. 5, under the title, "On the Voltaic Polarization of Solid and Fluid Bodies."

As some of the questions and suggestions stated in Mr. Grove's last paper have already been answered and appreciated, and one or two material points immediately bearing upon the recent researches of that ingenious and skilful experimenter are fully discussed in the memoir alluded to, it might, perhaps, interest those English philosophers who are not in the habit of reading German periodicals, to see in your excellent Magazine a translation of my paper, or some abstracts from it.

Before closing my letter, permit me to say a few words in reference to the voltaic part which, in my humble opinion, oxygen acts in the novel gaseous battery of Mr. Grove. According to my experiments, as stated in the paper beforementioned, an aqueous solution of hydrogen being voltaically

combined with chemically pure water, and the circuit thus formed completed by platinum, produces a current which passes from the solution to the water, though no trace of free oxygen should happen to be contained in either fluid. By letting pass either the latter gas or atmospheric air into the solution of hydrogen, I could not sensibly increase the current produced by the arrangement, from which negative result I thought I was entitled to draw the conclusion, that the current being generated under those circumstances cannot be due to the combination of free hydrogen with isolated oxygen; an inference which may also be drawn from Mr. Grove's arrangement itself, for the oxygen being contained in one tube cannot be supposed to combine with the hydrogen inclosed in another tube.

An aqueous solution of oxygen being voltaically associated with pure water is not capable of exciting a sensible current if the circuit happens to be completed by means of platinum, a fact from which it seems likewise to follow that in Grove's novel pile oxygen does not immediately contribute to the production of its current.

But if the current of that arrangement be, nevertheless, augmented by having the alternate glass-tubes filled with oxygen, I am inclined to ascribe that effect to the depolarizing action exerted by oxygen upon the negative platinum electrodes which are inserted in the tubes containing that gaseous body. From obvious reasons that action must be greatly facilitated and accelerated by the well-known power of platinum to favour the chemical union of hydrogen with oxygen.

It is, however, very likely that in the oxygen tubes, besides the depolarizing action, there is some other electromotive force called into play; but having treated of this subject in a paper ("On the electrolytical power of a simple pile") which, I presume, was read before the British Association at Manchester, and published in the last Number of De la Rive's *Archives*, I will not enter into more details on that subject, but take the liberty of referring to the memoir itself.

I remain, my dear Sir,

Yours very truly,

Bâsle, Dec. 28, 1842. C. F. Schœnbein.

P.S. Experimenters who are desirous of pursuing Mr. Grove's late researches, will find the effects of the gaseous pile greatly enhanced by making use of chlorine gas instead of oxygen; at least the experiments I made on the voltaic properties of chlorine and bromine, some time ago, lead to such a conclusion.

XXV. *Investigation of* Brianchon's *Theorem.* *By* STEPHEN FENWICK, *Esq., Royal Military Academy, Woolwich**.

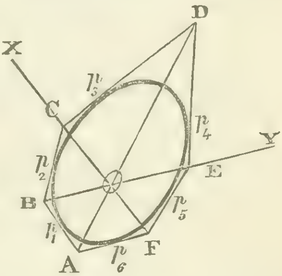
IF a hexagon be circumscribed to a conic section, the three diagonals which join the three pairs of opposite summits will pass through the same point.

This theorem, I believe, has not yet been completely established by the method of coordinates. Sir John Lubbock's investigation, which appeared in the Philosophical Magazine for August 1838, is adapted to the case of the parabola; and the mode of extending it to the other cases is pointed out. In the following investigation, the equation of the conic section in general is used, and the property is demonstrated on elementary principles alone.

Let p_1, p_2, p_3, &c. be the points of contact, which we shall denote by

$$x_1 y_1,\ x_2 y_2,\ x_3 y_3,\ x_4 y_4,\ x_5 y_5,\ x_6 y_6;$$

and A B, B C, C D, &c. the tangents at these points. Having joined C F, E B, intersecting in O, and also D O, A O; refer the system to C F, B E, as axes of coordinates, and denote the conic section by the general equation,



$$a y^2 + b x y + c x^2 + d y + e x + f = 0. \quad \ldots\ldots \quad \text{(A.)}$$

The several tangents will then be denoted by the equations,

$$\text{(A B)} \ldots y\,(2 a y_1 + b x_1 + d) + x\,(2 c x_1 + b y_1 + e) + d y_1 + e x_1 + 2 f = 0 \ldots (1.)$$
$$\text{(B C)} \ldots y\,(2 a y_2 + b x_2 + d) + x\,(2 c x_2 + b y_2 + e) + d y_2 + e x_2 + 2 f = 0 \ldots (2.)$$
$$\text{(C D)} \ldots y\,(2 a y_3 + b x_3 + d) + x\,(2 c x_3 + b y_3 + e) + d y_3 + e x_3 + 2 f = 0 \ldots (3.)$$
$$\text{(D E)} \ldots y\,(2 a y_4 + b x_4 + d) + x\,(2 c x_4 + b y_4 + e) + d y_4 + e x_4 + 2 f = 0 \ldots (4.)$$
$$\text{(E F)} \ldots y\,(2 a y_5 + b x_5 + d) + x\,(2 c x_5 + b y_5 + e) + d y_5 + e x_5 + 2 f = 0 \ldots (5.)$$
$$\text{(F A)} \ldots y\,(2 a y_6 + b x_6 + d) + x\,(2 c x_6 + b y_6 + e) + d y_6 + e x_6 + 2 f = 0 \ldots (6.)$$

Combining (3, 4.), and also (1. 6.), so that the absolute terms in the resulting equations may be eliminated, we get the equations of D O, A O.

$$\text{(D O)} \ldots y \left\{ \frac{2 a y_3 + b x_3 + d}{d y_3 + e x_3 + 2 f} - \frac{2 a y_4 + b x_4 + d}{d y_4 + e x_4 + 2 f} \right\} + x \left\{ \frac{2 c x_3 + b y_3 + e}{d y_3 + e x_3 + 2 f} - \frac{2 c x_4 + b y_4 + e}{d y_4 + e x_4 + 2 f} \right\} = 0 \ldots (7.)$$

$$\text{(A O)} \ldots y \left\{ \frac{2 a y_1 + b x_1 + d}{d y_1 + e x_1 + 2 f} - \frac{2 a y_6 + b x_6 + d}{d y_6 + e x_6 + 2 f} \right\} + x \left\{ \frac{2 c x_1 + b y_1 + e}{d y_1 + e x_1 + 2 f} - \frac{2 c x_6 + b y_6 + e}{d y_6 + e x_6 + 2 f} \right\} = 0 \ldots (8.)$$

It will suffice now to show that the products of the coefficients of y, x in (7.) and (8.), taken in a *cross order*, are equal

* Communicated by T. S. Davies, Esq., Royal Military Academy.

or rather, the products of the numerators, since the products of the denominators are evidently equal; for in that case the lines will make equal angles with the axis of x.

Performing the operations indicated by the signs with each of these coefficients, we get

$$\alpha = (2ae-bd)(y_3x_4-x_3y_4)+(4af-d^2)(y_3-y_4)+(2bf-dc)(x_3-x_4)\ldots \quad (9.)$$
$$\beta' = (2cd-be)(x_1y_6-x_6y_1)+(4cf-e^2)(x_1-x_6)+(2bf-de)(y_1-y_6)\ldots(10.)$$
$$\alpha' = (2ae-bd)(y_1x_6-x_1y_6)+(4af-d^2)(y_1-y_6)+(2bf-dc)(x_1-x_6)\ldots(11.)$$
$$\beta = (2cd-be)(x_3y_4-y_3x_4)+(4cf-e^2)(x_3-x_4)+(2bf-de)(y_3-y_4)\ldots(12.)$$

α, β being the coefficients of y and x in (7.), without the denominators, α', β' those of y and x in (8.).

Again, since the lines (2, 3.) meet in the axis of x; put $y=0$ in each of these, and equate the resulting values of x, thence we have the relation,

$$(2cd-be)(y_2x_3-y_3x_2)+(4cf-e^2)(x_3-x_2)+(2bf-ed)(y_3-y_2)=0\ldots(13.)$$

Reasoning in a similar way with the lines (5, 6.), (1, 2.), and (4, 5.), we get the following:

$$(2cd-be)(x_6y_5-x_5y_5)+(4cf-e^2)(x_6-x_5)+(2bf-ed)(y_6-y_5)=0\ldots(14.)$$
$$(2ae-bd)(y_1x_2-x_1y_2)+(4af-d^2)(y_1-y_2)+(2bf-ed)(x_1-x_2)=0\ldots(15.)$$
$$(2ae-bd)(y_4x_5-x_4y_5)+(4af-d^2)(y_4-y_5)+(2bf-ed)(x_4-x_5)=0\ldots(16.)$$

If, now, we substitute the values of $(2cd-be)$, $(2ae-bd)$ $(4cf-e^2)$, and $(4af-d^2)$ derived from (13, 14, 15, 16.) in (9, 10, 11, 12.), and multiply out, we shall get the identity

$$\alpha\beta' = \alpha'\beta,$$

hence A O, D O are in the same straight line. This establishes completely the theorem in question.

XXVI. *Demonstration of* Pascal's *Theorem relative to the Hexagon inscribed in a Conic Section. By* William Rutherford, *Esq., F.R.A.S., Royal Military Academy**.

If the three pairs of opposite sides of a hexagon inscribed in a conic section be produced to meet, the three points of intersection will range in the same straight line.

Let A B C D E F be a hexagon inscribed in a conic section, and let the sides A B, D E meet in G, the sides B C, E F in H, and the sides C D, A F in K. Take H for origin, H B, H F for the axes of positive coordinates of x and y respectively, and designate the coordinates of the six angular points C, B, E, F, A, D by $\alpha\,0$, α_1, $0\,\beta$, $0\,\beta_1$, $x_1\,y_1$, $x_2\,y_2$ respectively. Then the equations of A B, C D, D E, F A are

* Communicated by T. S. Davies, Esq., Royal Military Academy. On the subject of this Paper, see Phil. Mag. S. 3. vol. xxi. p. 37; and pres. vol. p. 31.

$$(\mathrm{A\,B}) \ldots \frac{x}{\alpha_1} + \frac{\alpha_1 - x_1}{\alpha_1 y_1} . y = 1 \ldots \quad (1.)$$

$$(\mathrm{C\,D}) \ldots \frac{x}{\alpha} + \frac{\alpha - x_2}{\alpha y_2} . y = 1 \ldots \quad (2.)$$

$$(\mathrm{D\,E}) \ldots \frac{y}{\beta} + \frac{\beta - y_2}{\beta x_2} . x = 1 \ldots \quad (3.)$$

$$(\mathrm{F\,A}) \ldots \frac{y}{\beta_1} + \frac{\beta_1 - y_1}{\beta_1 x_1} . x = 1 \ldots \quad (4.)$$

Subtracting (3.) from (1.) and (4.) from (2.), we obtain the equations of G H and H K.

$$(\mathrm{G\,H}) \ldots \left\{ \frac{1}{\alpha_1} - \frac{\beta - y_2}{\beta x_2} \right\} x + \left\{ \frac{\alpha_1 - x_1}{\alpha_1 y_1} - \frac{1}{\beta} \right\} y = 0 \ldots (5.)$$

$$(\mathrm{H\,K}) \ldots \left\{ \frac{1}{\alpha} - \frac{\beta_1 - y_1}{\beta_1 x_1} \right\} x + \left\{ \frac{\alpha - x_1}{\alpha y_2} - \frac{1}{\beta_1} \right\} y = 0 \ldots (6.)$$

And if X Y be the coordinates of H, and X_1 Y_1 those of K; then from (5.) and (6.) we get

$$\frac{X}{Y} = \frac{(\alpha_1 y_1 + \beta x_1 - \alpha_1 \beta)\, x_2}{(\alpha_1 y_2 + \beta x_2 - \alpha_1 \beta)\, y_1} \text{ and } \frac{X_1}{Y_1} = \frac{(\alpha y_2 + \beta_1 x_2 - \alpha \beta_1)\, x_1}{(\alpha y_1 + \beta_1 x_1 - \alpha \beta_1)\, y_2}.$$

Now the coordinates of the points C, B, E, F, A, D being substituted in the equation of the conic section,

$$y^2 + b\,x\,y + c\,x^2 + d\,y + e\,x + f = 0$$

will give the six subsequent equations, viz.

$$c\,\alpha^2 + e\,\alpha + f = 0 \ldots (7.) \quad \Big| \quad \beta^2 + d\,\beta + f = 0 \ldots (9.)$$
$$c\,\alpha_1^2 + e\,\alpha_1 + f = 0 \ldots (8.) \quad \Big| \quad \beta^2_1 + d\,\beta_1 + f = 0 \ldots (10.)$$

$$y_1^2 + b\,x_1\,y_1 + c\,x_1^2 + d\,y_1 + e\,x_1 + f = 0 \ldots (11.)$$
$$y_2^2 + b\,x_2\,y_2 + c\,x_2^2 + d\,y_2 + e\,x_2 + f = 0 \ldots (12.)$$

From (7.), (8.), (9.), (10.) we derive

$$e = -c\,(\alpha + \alpha_1);\ f = c\,\alpha\,\alpha_1;\ d = -(\beta + \beta_1), \text{ and } f = \beta\,\beta_1$$

$$\therefore\ c = \frac{\beta\,\beta_1}{\alpha\,\alpha_1} \text{ and } e = -\frac{\beta\,\beta_1}{\alpha\,\alpha_1}(\alpha + \alpha_1).$$

Multiply equation (11.) by $x_2\,y_2$ and (12.) by $x_1\,y_1$, and subtract; then we get

$$x_2\,y_2\,(y_1^2 + c\,x_1^2 + d\,y_1 + e\,x_1 + f) = x_1\,y_1\,(y_2^2 + c\,x_2^2 + d\,y_2 + e\,x_2 + f),$$

which by substituting the preceding values of c, d, e, f is transformed to

$$\frac{x_1\,y_1}{x_2\,y_2} = \frac{\alpha\,\alpha_1\,y_1^2 + \beta\,\beta_1\,x_1^2 - \alpha\,\alpha_1\,(\beta + \beta_1)\,y_1 - \beta\,\beta_1\,(\alpha - \alpha_1)\,x_1 + \alpha\,\alpha_1\,\beta\,\beta_1}{\alpha\,\alpha_1\,y_2^2 + \beta\,\beta_1\,x_2^2 - \alpha\,\alpha_1\,(\beta + \beta_1)\,y_2 - \beta\,\beta_1\,(\alpha + \alpha_1)\,x_2 + \alpha\,\alpha_1\,\beta\,\beta_1}$$

and since $\frac{x_1 y_1}{x_2 y_2} = \frac{(\alpha\beta + \alpha_1\beta_1)\, x_1 y_1}{(\alpha\beta + \alpha_1\beta_1)\, x_2 y_2}$; consequently we have

$$\frac{x_1 y_1}{x_2 y_2} = \frac{\alpha\alpha_1 y_1^2 + (\alpha\beta + \alpha_1\beta_1)\, x_1 y_1 + \beta\beta_1 x_1^2 - \alpha\alpha_1 (\beta + \beta_1)\, y_1 - \beta\beta_1 (\alpha + \alpha_1)\, x_1 + \alpha\alpha_1\beta\beta_1}{\alpha\alpha_1 y_2^2 + (\alpha\beta + \alpha_1\beta_1)\, x_2 y_2 + \beta\beta_1 x_2^2 - \alpha\alpha_1 (\beta + \beta_1)\, y_2 - \beta\beta_1 (\alpha + \beta_1)\, x_2 + \alpha\alpha_1\beta\beta_1}$$

$$= \frac{(\alpha y_1 + \beta_1 x_1 - \alpha\beta_1)(\alpha_1 y_1 + \beta x_1 - \alpha_1\beta)}{(\alpha y_2 + \beta_1 x_2 - \alpha\beta_1)(\alpha_1 y_2 + \beta x_2 - \alpha_1\beta)};$$

therefore, finally,

$$\frac{(\alpha_1 y_1 + \beta x_1 - \alpha_1\beta)\, x_2}{(\alpha_1 y_2 + \beta x_2 - \alpha_1\beta)\, y_1} = \frac{(\alpha y_2 + \beta_1 x_2 - \alpha\beta_1)\, x_1}{(\alpha y_1 + \beta_1 x_1 - \alpha\beta_1)\, y_2}.$$

But these are the values of $\frac{X}{Y}$ and $\frac{X_1}{Y_1}$ obtained above, and therefore

$$X\, Y_1 - X_1\, Y = 0,$$

which is the criterion of the origin H, and the points C and K being in the same straight line, therefore the points of intersection G, H, K range in the same straight line.

XXVII. *On the Action of the Rays of the Solar Spectrum on Vegetable Colours, and on some new Photographic Processes. By* Sir John F. W. Herschel, *Bart., K.H., F.R.S.*

[Continued from p. 116.]

202. I SHALL conclude this part of my subject by remarking on the great number and variety of substances which, now that attention is drawn to the subject, appear to be photographically impressible. It is no longer an insulated and anomalous affection of certain salts of silver and gold, but one which doubtless, in a greater or less degree pervades all nature, and connects itself intimately with the mechanism by which chemical combination and decomposition is operated. The general instability of organic combinations might lead us to expect the occurrence of numerous and remarkable cases of this affection among bodies of that class, but among metallic and other elements inorganically arranged, instances enough have already appeared, and more are daily presenting themselves, to justify its extension to all cases in which chemical elements may be supposed combined with a certain degree of laxity, and so to speak, in a state of tottering equilibrium. There can be no doubt that the process, in a great majority if not all the cases which have been noticed among inorganic substances, is a deoxidizing one, so far as the more refrangible rays are concerned. It is obviously so in the cases of gold and silver. In that of the bichromate of potash it is most probable that an atom of oxygen is parted with, and so of many others. A beautiful example of such deoxidizing action on a

non-argentine compound has lately occurred to me in the examination of that interesting salt, the ferrosesquicyanuret of potassium, described by Mr. Smee in the Philosophical Magazine, [S. 3.] No. 109, September 1840, and which he has shown how to manufacture in abundance and purity by voltaic action on the common, or yellow ferrocyanuret. In this process nascent oxygen is absorbed, hydrogen given off, and the characters of the resulting compound in respect of the oxides of iron, forming as it does Prussian blue with protosalts of that metal, but producing no precipitate with its persalts, indicate an excess of electro-negative energy, a disposition to part with oxygen, or, which is the same thing, to absorb hydrogen (in the presence of moisture), and thereby to return to its pristine state, under circumstances of moderate solicitation, such as the affinity of protoxide of iron (for instance) for an additional dose of oxygen, &c.

203. Paper simply washed with a solution of this salt is highly sensitive to the action of light. Prussian blue is deposited (the base being necessarily supplied by the destruction of one portion of the acid, and the acid by the decomposition of another). After half an hour or an hour's exposure to sunshine, a very beautiful negative photograph is the result, to fix which all that is necessary is to soak it in water, in which a little sulphate of soda is dissolved, to ensure the fixity of the Prussian blue deposited. While dry, the impression is dove-colour or lavender blue, which has a curious and striking effect on the greenish yellow ground of the paper produced by the saline solution. After washing, the ground colour disappears, and the photograph becomes bright blue on a white ground. If too long exposed it gets "oversunned," and the tint has a brownish or yellowish tendency, which however is removed in fixing: but no increase of intensity beyond a certain point is obtained by continuance of exposure.

204. Prismatic examination of this process demonstrates the remarkable and valuable fact, that the decomposition of the salt and deposit of Prussian blue is due to the action of the blue and violet rays, the less refrangible rays below the blue having absolutely no influence either to exalt or diminish the effect. The limits of action are about + 18·0 and + 61·0, fading insensibly both ways. The greatest intensity of action is at + 38. A feebler maximum occurs at + 23. The intensity of the impression is much increased by washing with acidulated water, still more if it hold in solution a little persalt of iron; but in this case the ground, if not very carefully defended from light, is blue.

205. If a solution of this salt, mixed with perchloride of

iron in a certain proportion, be washed over paper somewhat bibulous and exposed to the spectrum, a copious and intense deposit of Prussian blue takes place over the region indicated in the last article. But it does not terminate there. On the contrary, the action is continued downwards in the spectrum, not only down to and beyond the extreme red rays, but far below, *down to the very end of the thermic spectrum* (as far as the spot called δ in Art. 136, and even with some traces of the more remote spot ε). The formation of the deposited colour in this region is accompanied with very singular phænomena, referable obviously to the heat developed by the thermic spectrum. Soon after the blue train, *a b*, fig. 9, [Plate II.], in the positive region of the spectrum is formed, and has begun to acquire some intensity, an oval α, blunt at one extremity and pointed at the other, and of a dark brown colour, begins to appear. It enlarges rapidly, and at the same time throws forth a projection β, indicating the action of that portion of the thermic spectrum so characterized in Art. 136. It also acquires a whitish narrow border, indicated by the dotted line, and very conspicuous on the green ground of the paper. The action continuing, the spot γ is marked out by the extension of the border in that direction, soon after which the spot appears, in brown. Lastly appears δ with feeble traces of further irregular and interrupted action. Measurements of these spots as they appear, leave no doubt of their identity in situation with the thermic spots α, β, γ, δ of Art. 136, and that they are referable to the drying of the paper is shown by the fact, that a film of the liquid dried in a porcelain saucer changes from green to dark brown at a definite point of dryness. Moreover, on wetting the paper, the brown spots disappear, and in their place we find a train of Prussian blue, of varying intensity, but *of uniform breadth* (not swelling and contracting, as is the case with the heat-spots formed by simple drying, and *therefore* obviously due to *direct* radiation), and terminating in two insulated and tolerably well-defined circular spots or solar images, holding precisely the places of γ and δ (viz. at $-35{\cdot}7$ and $-45{\cdot}1$).

206. If in lieu of the perchloride of iron, we substitute a solution of that curious salt the *ammonio-citrate of iron*, the photographic effects are among the most various and remarkable that have yet offered themselves to our notice in this novel and fertile field of inquiry. The two solutions mix without causing any precipitate, and produce a liquid of a brown colour, which washed over paper is green (being strongly dichromatic). If this be done under the prism, the action of the spectrum is almost instantaneous, and most intense. A

copious and richly coloured deposit of Prussian blue is formed over the whole of the blue, violet, and extra-spectral rays in that direction, extending downwards (with rapid graduation) almost to the yellow. If arrested when the blue is most intense and thrown into water, the impression is fixed, as in the accompanying specimen (see fig. 10.). But if the action of the light be continued, strange to say, the blue and violet rays begin to destroy their own work. A *white* oval makes its appearance in the most intense part of the blue (fig. 11.), which extends rapidly upwards and downwards. At a certain point of the action, the upper or more refrangible extremity of the white impression exhibits a semicircular termination, beyond which is a distinct and tolerably well-defined *conjugate image*, or insulated circular white spot, whose centre is situated far beyond the extreme visible violet.

207. If paper washed over with the mixed solution in question is exposed wet to sunshine, it darkens to a livid purple and rapidly whitens again. If the exposure be continued, the white again darkens gradually to a brownish violet hue. But in the shade it slowly resumes its original tint, after which it is again and again susceptible of the same round of action. The most singular and apparent capricious varieties of coloration and discoloration however arise (as is so frequently the case in photographic experiments) from different dosage of ingredients, order of washes, &c., so as to make the study of the phænomena in a high degree complicated *. A certain adjustment of proportions gives an exquisite and highly sensitive *positive* photographic paper; another, a negative one, in which the impression of light, feeble at first, is strongly brought out afterwards by an additional wash of the ferrosesquicyanuret, &c.

208. The ordinary ferrocyanuret (the yellow salt), though not nearly so sensible to photographic action, is yet far from inert. In my former paper I have noticed its property of fixing against the further action of light, and ultimately destroying, photographic impressions on argentine papers. In conjunction also with preparations of silver, it has been made by Mr. Hunt the basis of a highly sensitive photographic paper. Its habitudes *per se* are, however, not a little remarkable. Paper simply washed with its fresh solution and ex-

* The whitening is very obviously due to the deoxidation of the precipitated Prussian blue and the formation of the proto-ferrocyanuret of iron; the resumption of colour in the shade, to the re-oxidizement of this compound, which is well known to absorb oxygen from the air with avidity. Simple Prussian blue, however, is not whitened by the violet rays. Its state must be peculiar. (See Postscript.)

posed to the spectrum, slowly receives a deposit of Prussian blue over the region of the blue, violet, and "lavender" rays: but this never becomes intense; another series of changes commencing, indicated by the formation of a violet-coloured streak within the blue, just where the violet itself is most intense in the spectrum. If the solution be very feebly acidulated with sulphuric acid, the first portion only of the spectral impression (from + 13·3 to + 20·0) is blue, the whole of the remainder (extending to + 51) snuff brown. The dose of acid being increased, the exposure prolonged, and the liquid plentifully supplied, a green thermic impression is produced by the less refrangible rays, in which the spots α, β, γ are very distinct, and lie exactly (by measure) in their proper places. This impression continues as far as the zero point, where it begins to pass into blue, and graduates insensibly into the photographic spectrum, which attains its maximum of blue at + 25, and is thence prolonged onwards as a dull bluish streak on a brown ground, somewhat broader than itself, and projecting like a border on both sides.

209. If paper be washed with a solution of ammonio-citrate of iron and dried, and then a wash passed over it of the yellow ferrocyanuret of potassium, there is no immediate formation of true Prussian blue, but the paper rapidly acquires a violet purple colour, which deepens after a few minutes, as it dries, to almost absolute blackness. In this state it is a positive photographic paper of high sensibility, and gives pictures of great depth and sharpness, but with this peculiarity, that they darken again spontaneously on exposure to air in darkness, and are soon obliterated. The paper, however, remains susceptible to light and capable of receiving other pictures, which in their turn fade, without any possibility (so far as I can see) of arresting them; which is to be regretted, as they are very beautiful, and the paper of such easy preparation. If washed with ammonia or its carbonate, they are for a few moments entirely obliterated, *but presently reappear, with reversed lights and shades.* In this state they are fixed, and the ammonia, with all that it will dissolve, being removed by washing in water, their colour becomes a pure Prussian blue, which deepens much by keeping. If the solutions be mixed there results a very dark violet-coloured ink, which may be kept uninjured in an opake bottle, and will readily furnish, by a single wash, at a moment's notice, the positive paper in question, which is most sensitive when wet.

210. It seems at first sight natural to refer these curious and complex changes to the instability of the cyanic compounds, and that this opinion is to a certain extent correct, is

proved by the photographic impressions described in Arts. 204 and 209, where no iron is added beyond what exists in the ferrocyanic salts themselves. Nevertheless the following experiments abundantly prove that in several of the changes above described, the *immediate action* of the solar rays is not exerted on these salts, but on the iron contained in the ferruginous solution added to them, which it deoxidizes or otherwise alters, thereby presenting it to the ferrocyanic salts in such a form as to precipitate the acids in combination with the peroxide or protoxide of iron, as the case may be. To make this evident, all that is necessary is *simply to leave out the ferrocyanate* in the preparation of the paper, which thus becomes reduced to a simple washing over with the ammonio-citric solution. Paper so washed is of a bright yellow colour, and is apparently little, but in reality highly sensitive to photographic action. Exposed to strong sunshine for some time indeed, its bright yellow tint is dulled into an ochrey hue, or even to gray, but the change altogether amounts to a moderate per-centage of the total light reflected, and in short exposures is such as would easily escape notice. Nevertheless, if a slip of this paper be held for only four or five seconds in the sun (the effect of which is quite imperceptible to the eye), and when withdrawn into the shade be washed over with the ferrosesquicyanate of potash, a considerable deposit of Prussian blue takes place on the part sunned, and none whatever on the rest, so that on washing the whole with water, a pretty strong blue impression is left, demonstrating the reduction of iron in that portion of the paper to the state of protoxide. The effect in question is not, it should be observed, peculiar to the ammonio-citrate of iron. The ammonio- and potasso-tartrate fully possess, and the perchloride *exactly neutralized* partakes of the same property: but the experiment is far more neatly made and succeeds better with the other salts.

211. If a long strip of paper, prepared as in the last article, be marked off into compartments and subjected to graduated exposure to sunshine, so that the times of exposure in each succession shall form an arithmetical progression of 1^m, 2^m, &c., and when withdrawn washed over as aforesaid with the ferrosesquicyanuret and rinsed in water, the blue deposit is found to increase with the time of exposure up to a very deep and full colour, after which its total intensity, so far from increasing, diminishes, and at length almost vanishes. Again, if a slip of the same paper be exposed a long while to the spectrum, the whole impression consists in a feeble ochrey-brown streak, extending over the region of the blue, violet and lavender rays as far as about $+ 55$. But on the application of the cyanic

solution (in the shade) a most intense blue spectrum is developed over the whole of the more refrangible region, in the interior of which the blue colour appears to have been, as it were, eaten away, leaving a white oval, as in the specimen annexed; precisely the same phænomenon, in short, as would have been produced under the spectrum had the two liquids acted in conjunction. And this white portion comports itself under the influence of water or air, just as it would have done had it been produced under such joint action; i. e. it gradually turns blue till it is no longer distinguishable from the rest of the spectrum. It is also blued by ammonia, just as the positive paper of Art. 210, after bleaching, would be, &c. In short, it is evident that we have succeeded in separating the final action described in that article into two distinct steps or stages, the photographic influence being confined to the first, and the ferrosesquicyanate acting as a mere precipitant on the nascent compounds resulting from that influence.

212. In order to ascertain whether any portion of the iron in the double ammoniacal salt employed had really undergone deoxidation, and become reduced to the state of protoxide as supposed, I had recourse to a solution of gold, exactly neutralized by carbonate of soda. The proto-salts of iron, as is well known to chemists, precipitate gold in the metallic state. The effect proved exceedingly striking, issuing in a process no wise inferior in the almost magical beauty of its effect to the calotype process of Mr. Talbot, which in some respects it nearly resembles, with this advantage, as a matter of experimental exhibition, that the disclosure of the dormant image does not require to be performed in the dark, being not interfered with by moderate daylight. As the experiment will probably be repeated by others, I shall here describe it *ab initio*. Paper is to be washed with a moderately concentrated solution of ammonio-citrate of iron, and dried. The strength of the solution should be such as to dry into a good yellow colour, not at all brown. In this state it is ready to receive a photographic image, which may be impressed on it either from nature in the camera-obscura, or from an engraving on a frame in sunshine. The image so impressed, however, is very faint, and sometimes hardly perceptible. The moment it is removed from the frame or camera, it must be washed over with a neutral solution of gold of such strength as to have about the colour of sherry wine. Instantly the picture appears, not indeed at once of its full intensity, but darkening with great rapidity up to a certain point, depending on the strength of the solutions used, &c. At this point nothing can surpass the sharpness and perfection of detail of the resulting

photograph. To arrest this process and to fix the picture (so far at least as the further agency of *light* is concerned), it is to be thrown into water very slightly acidulated with sulphuric acid and well soaked, dried, washed with hydrobromate of potash, rinsed, and dried again.

213. Such is the outline of a process to which I propose applying the name of *Chrysotype*, in order to recal by similarity of structure and termination the *Calotype* process of Mr. Talbot, to which in its general effect it affords so close a parallel. Being very recent, I have not yet (June 10, 1842) obtained a complete command over all its details, but the termination of the Session of the Society being close at hand, I have not thought it advisable to suppress its mention. In point of *direct* sensibility, the Chrysotype paper is certainly inferior to the Calotype; but it is one of the most remarkable peculiarities of gold as a photographic ingredient, that *extremely feeble impressions once made by light, go on afterwards darkening spontaneously, and very slowly, apparently without limit, so long as the least vestige of unreduced chloride of gold remains in the paper**. To illustrate this curious and (so far as applications go) highly important property, I shall mention (incidentally) the results of some experiments made during the late fine weather, on the habitudes of gold in presence of oxalic acid. It is well known to chemists that this acid heated with solutions of gold precipitates the metal in its metallic state; it is upon this property that Berzelius has founded his determination of the atomic weight of gold. Light, as well as heat, also operates this precipitation; but to render it effectual, several conditions are necessary:—1st, the solution of gold must be neutral, or at most *very* slightly acid; 2nd, the oxalic acid must be added in the form of a neutral oxalate; and 3rdly, it must be present in a certain considerable quantity, which quantity must be greater, the greater the amount of free acid present in the chloride. Under these conditions, the gold is precipitated by light as a black powder if the liquid be in any bulk, and if merely washed over paper a stain is produced, which, however feeble at first, under a certain dosage of the chloride, oxalate, and free acid, goes on increasing from day to day and from week to week, when laid by in the dark, and especially in a damp atmosphere, till it acquires almost the blackness of ink; the unsunned portion of the paper remaining unaffected, or so slightly as to render it almost

* Subsequent experiments have convinced me that this property cannot be taken advantage of to increase the intensity of the chrysotype impression, however it may be available in other processes. Note added during the printing, J. F. W. H.

certain that what little action of the kind exists is due to the effect of casual dispersed light incident in the preparation of the paper. I have before me a specimen of paper so treated, in which the effect of thirty seconds exposure to sunshine was quite invisible at first, and which is now of so intense a purple as may well be called black, while the unsunned portion has acquired comparatively but a very slight brown. And (which is not a little remarkable, and indicates that in the time of exposure mentioned the *maximum* of effect was attained) other portions of the same paper exposed in graduated progression for longer times, viz. 1^m, 2^m, and 3^m, are not in the least perceptible degree darker than the portion on which the light had acted during thirty seconds only.

214. The very remarkable phænomenon, described in Art. 208. of a second darkening, different in character and colour, coming on after the bleaching effect of solar light has been fully completed, is not without a parallel among the argentine compounds. I refer to the action of the hydriodic salts on argentine papers completely blackened by exposure to sunshine, an action imperfectly described in § 5. of my former paper (Art. 94 *et seq.*), and signalized as to one of its most striking peculiarities in Note 2, Art. 129. of that communication. To study the phænomena of this action in their simplest form, a paper prepared without iodine, and of a positive character is required. The simplest and most convenient is that prepared by Mr. Hunt with one wash of muriate of ammonia, two of nitrate of silver*, and exposure to sunshine. With such paper (obligingly furnished me by Mr. Hunt himself) I made the following experiments.

215. Exposed to the spectrum and washed with a solution of hydriodate of potash too weak fully to excite it †, two contrary actions were produced by the rays above and below the zero point or mean yellow. By the former the paper began to be bleached at a point distant + 26·5 parts from the zero, from which point the bleaching extended gradually upwards to a considerable distance, and downwards to the circumference of a semicircle, having that point for a centre. By the latter the paper was darkened (at least in comparison with its general surface, which, purposely subjected to dispersed light, had begun to lose much of its original intense blackness), the darkness spreading also upwards and downwards: upwards till it passed the zero point, and nearly or quite attained the semicircle above mentioned; and downwards to about − 19, or − 20 parts. As

* Muriate of ammonia forty grains, water four ounces; nitrate of silver sixty grains, water one ounce.

† Ioduret of potassium sixty grains, water one ounce.

the paper dried the action seemed to be suspended. It became therefore necessary to renew the hydriodic wash, and thereby to increase the actual quantity of that salt present on the paper. Both actions grew more intense, but the bleaching effect most so. A perfect semicircle and long cometic train, *c*, *d*, fig. 12, No. 1, (Plate II.), was produced, within which space the blackness of the paper was totally destroyed, and replaced by white or rather very pale yellow. The hydriodic washes being again and again renewed, the darkness at first produced in the lower part of the spectrum began to give way, and was slowly replaced by a very feeble bleaching, which at length extended very far indeed below the extreme red rays, and upwards to join the semicircle C fig. 12, No. 2, which had by this time assumed an outline perfectly sharp and well-defined, having its centre on the original point + 26·5 of its commencement. But *within* this semicircle and its train, remarkable changes were observed to be all the while in progress. First, a somewhat dark, and grayish or brownish, perfectly circular and well-defined solar image arose, its diameter being somewhat less than that of the semicircular terminations, *so as to leave a clear and distinct white border all around it*, as represented by the dotted line in fig. 12, No. 2. Shortly after the complete formation of this spot, *i. e.* after its circular outline could be distinctly traced all round, it began to extend itself upwards into an oval or tailed form, but preserving its circular shape below and maintaining the white border inviolate, assuming at the same time a brownish yellow colour which gradually deepened, but never became intense. At the same time a very remarkable change was observed to take place in the reflective (or absorbent) powers of the paper in this region. The violet-coloured end of the spectrum, which hitherto had been distinctly seen as usual occupying the space from + 30 to + 40·6, became quite indiscernible, while on the other hand the blue rays adjoining became reflected with such copiousness as to terminate the spectrum by a well-defined semicircle *c*, fig. 12, No. 3, and to give to the whole portion *c c* the appearance of a brilliant and purely blue spot. Finally, after long-continued action, the interior browned oval above-mentioned was found to have been prolonged into a figure of the form No. 4, fig. 12 (distinctly seen at the *back* of the paper), of which the termination by a narrow neck and circular enlargement indicates the definite action of a ray much further removed along the axis of the spectrum. Washing with water at once obliterates this part of the phænomenon, destroys the brown colour, and leaves simply the bleached cometic train, in singularly striking con-

trast with the dark ground of the paper. Specimens of the spectrum itself are subjoined for inspection.

216. The black positive paper used in the above experiment (which has been often repeated with the same results) contains free nitrate of silver. If this be washed out, the darkening at the lower end of the spectrum is not produced, but in its place the feeble subsequent bleaching in the region above-mentioned commences at once. And if besides washing with mere water, the paper be subsequently washed with a neutral hyposulphite to remove all chloride of silver, it is reduced to a state of perfect insensibility. It is therefore to this latter element that the direct action of the bleaching rays is to be referred. A few months' keeping also destroys the positive sensibility of the paper in question entirely.

Collingwood, June 13, 1842. J. F. W. HERSCHEL.

[The Postscript of August 29 will follow in continuation.]

XXVIII. *On Kakodylic Acid, and the Sulphurets of Kakodyle. By* Professor BUNSEN*.

IN my former researches† on the kakodyle compounds I proceeded upon the supposition that the base contained in them was a ternary radical having the formula $C^4 H^6 As^2$; and I have done myself the honour of laying before the last meeting at Plymouth, the results of a very tedious and at the same time dangerous series of researches, by which I have proved that this radical can not only be separated from its compounds, but that it also possesses the property in common and in a precisely similar manner with the simple metals, of combining directly with the other bodies. If, then, on the one hand we arrive at the firm conviction that the theory of organic radicals is no longer a hypothetical fiction, but the expression of facts which do not allow of any other interpretation, the study of the kakodyle series acquires on the other hand an importance with respect to the theory of the science which calls for the most careful examination of its compounds. I have therefore directed my attention to the higher compounds of the radical, and have arrived at results no less interesting, and to which I should the more wish to direct the attention of this meeting, as they stand in exact opposition to the views which the new French school of chemistry has endeavoured to introduce into the science.

* Read before the Chemical Section of the British Association at the Manchester Meeting and communicated by Prof. Croft.

† See Phil. Mag. S. 3. vol. xx. p. 382, 395.—EDIT.

1. *Kakodylic Acid.*

The production of this curious body, which I formerly called *Alkargen*, depends upon one of the most uncommon phænomena of organic chemistry. It is formed namely by the direct oxidation of the radical or of its protoxide. If the former is gradually brought into contact with oxygen, this gas being allowed to come in contact with it so slowly that inflammation cannot occur, it absorbs one atom of oxygen and is converted into oxide of kakodyle. If the air be still allowed to act on this body under similar precautions, part of the oxide takes up 2 atoms more oxygen, forming an acid which combines with the excess of oxide, producing a new state of oxidation of the nature of a salt, and which, doubtless, corresponds to the hyponitric acid $NO + NO_3$ or $N^2 O^4$. This compound, which cannot however be completely freed from excess either of oxide or of acid, forms a tenacious thick fluid, which is less soluble in water than the acid, but more so than the oxide, and which when distilled is resolved into these two bodies. If this tenacious liquid be warmed to 50° C. and a current of oxygen passed through it for several days, it is finally converted into kakodylic acid, which may be purified by pressure between bibulous paper and repeated crystallization.

The behaviour of this radical is therefore in exact contradiction to the premises of Dumas' theory of substitution*, and is perfectly similar to that of a simple metal, which when exposed to the influence of oxygen runs through all the intermediate steps of oxidation until it reaches the highest, as is seen in the accompanying table:—

$C^4 H^6 As^2$	Free radical.		
$C^4 H^6 As^2 O$ . . .	1st product of the action of oxygen.		
$C^4 H^6 As^2 O^2$ . . .	2nd ...	...	...
$HO + C^4 H^6 As^2 O^3$	3rd ...	...	...

The preparation of kakodylic acid by direct oxidation of the oxide is rendered both disagreeable and dangerous by the great inflammability of this substance, and by its stupifying odour. I have therefore endeavoured to discover a simpler method of preparation, and oxide of mercury is extremely well-suited for the purpose; for when this substance is digested under water with oxide of kakodyl, it converts the whole of it in a few seconds into kakodylic acid, which may be purified by a single recrystallization from alcohol. 76 grammes of oxide of kakodyle which had not been freed from water, gave when treated in this manner 88 grs. of the hydrated acid. If the oxide had been anhydrous it should have given 92·7 grms.,

* See Phil. Mag. S. 3. vol. xvi. p. 322.—Edit.

and consequently the experiment agrees exceedingly well with the theory.

Kakodylic acid forms large, glassy, perfectly transparent crystals, which are oblique quadrilateral prisms with oblique-angled unequal terminal faces; they belong to the trimetric system. The substance is not altered by exposure to dry air, but is decomposed by moisture. It is less soluble in absolute alcohol than in water, but not at all soluble in æther. Among all the kakodyle compounds it is the only one which does not possess the least smell. In a toxicological point of view this substance is very remarkable; for although it contains more than 72 per cent of arsenic, and oxygen in the same proportion as arsenious acid, it does not exhibit the least poisonous properties: 8 grains dissolved in water were injected into the jugular vein of a rabbit, but produced neither death, nor indeed any symptom of a poisonous action. This unexpected fact is in perfect concordance with one which has not been as yet regarded, but which is evident in the pharmaco-dynamical properties of organic bodies, and on which is founded one of the characteristic distinctions between the inorganic bodies and those produced by the interference of vitality. If namely certain matters are added to inorganic bodies, their pharmaco-dynamical effects are thereby somewhat altered, but not destroyed. If, on the contrary, they combine together to form organic substances, these properties are completely lost. Copper, mercury, lead and barium do not lose their poisonous qualities, whatever the *soluble* compound may be in which they are present. Carbon, hydrogen, nitrogen, and oxygen, which in strychnine, atropine and coniine form the most violent poisons, are altogether harmless in the compounds of proteine. This fact receives a beautiful confirmation from the kakodylic acid; arsenic as if combined by organic affinity has become a harmless body. Kakodylic acid contains to 1 atom of radical 4 atoms of oxygen, of which 1 atom is combined with hydrogen to form basic water, which cannot be drawn out by heat, but only by stronger bases. Its formula is $C^4 H^6 As^2 O^3 + H^2 O$, which has been deduced from new analyses made with chromate of lead: the formula which I had previously assumed was $C^4 H^6 As^2 O^4 + H^2 O$. The analyses were made with oxide of copper, and were therefore incorrect, for kakodylic acid is one of the most difficultly combustible substances, and cannot be completely burnt with oxide of copper.

The acid can support a very high temperature without undergoing decomposition: this commences at 200° C. The radical in this compound seems to possess a much greater stability than in the oxide; I have observed that the acid in this

respect surpasses even the succinic acid. Concentrated nitric acid, even nitromuriatic acid, or indeed a mixture of the chromic and sulphuric acids, exerts no action upon it even at a boiling temperature: if the excess of the first-named acid be driven off by evaporation, a thick syrupy liquid remains, which is probably $NO^5 + C^4 H^6 As^2 O^3$, which can bear a higher temperature than NO^5 without being decomposed, and when heated still more strongly burns with a slight explosion.

That, however, which renders this body so very interesting for the theory, is the great facility with which its reduction may be effected; phosphorous acid abstracts 2 atoms of oxygen and produces the oxide. Protochloride of tin acts in the same way, except that the chloride of the radical is formed; but what appears to me still more remarkable is, that this reduction may be effected by means of metallic zinc, kakodylate of zinc being formed.

This acid is very weak; it certainly does decompose the carbonates, but only with slowness and difficulty. Its salts are all soluble in water, and may be crystallized from alcohol. Oxide of silver forms three salts; the neutral one, which crystallizes from its alcoholic solution in long fine silky needles, is obtained by dissolving pure oxide of silver in kakodylic acid; the salt is not changed in the air unless light is admitted; when heated red-hot it leaves metallic silver perfectly free from arsenic. It is anhydrous, and has the formula $C^4 H^6 As^2 O^3 + Ag\,O$.

By treating an aqueous solution of kakodylic acid with carbonate of silver, another salt somewhat similar in appearance to the above is obtained. It is a ter-acid salt, and is formed because the terkakodylate of silver cannot further decompose the carbonate. The formula is $3\,(C^4 H^6 As^2 O^3) + Ag\,O$. The third compound is a double salt of kakodylate and nitrate of silver. It is obtained by mixing together alcoholic solutions of kakodylic acid and nitrate of silver; its formation is accompanied by a very remarkable phænomenon. First of all the neutral salt is separated in the form of silky needles, which in a few moments change into silky scales while still under the liquid. This salt blackens with great rapidity in the air, and explodes when heated. Its composition is

$$Ag\,O,\ NO^5 + Ag\,O,\ C^4 H^6 As^2 O^3.$$

The salt of mercury only crystallizes when an excess of kakodylic acid is present: it forms exceedingly fine silky needles, which aggregate in star-shaped masses. It is decomposed by water, oxide of mercury being separated. The potassa salt is soluble in water, but not in absolute alcohol and æther. It crystallizes in concentric radiated groups of crystals like

Wavellite. If an alcoholic solution of the acid be boiled with an alcoholic solution of chloride of copper, a greenish yellow pulverulent precipitate is formed, which consists of 7 atoms of chloride of copper and 1 atom of bikakodylate of copper.

Sulphurets of Kakodyl.

Kakodyle combines directly with 1 atom of sulphur, when both substances in a perfectly dry state are brought together; the same compound may be formed by distilling chloride of kakodyl with sulphuret of barium. This compound is, as I have already stated, fluid, and does not solidify at a very low temperature. If more sulphur is added another atom of it is taken up by the radical, and it becomes a white solid mass soluble in æther, from which it can be obtained in large, oblique quadrilateral prisms. This compound possesses the property of combining with more sulphur when the dry substance is digested with it: it forms a confused mass of acicular crystals. The behaviour of sulphuretted hydrogen with the kakodylates of the alkaline bases, renders it very probable that this acicular compound is analogous in composition to the kakodylic acid; it can only exist in an anhydrous state, and by treatment with solvents is decomposed into bisulphuret of kakodyl and sulphur.

The radical kakodyle is therefore capable of combining directly with sulphur, forming two, if not three sulphurets, which are perfectly analogous to the three oxides, as is shown in the following table:—

$C^4 H^6 As^2$	Radical.
$C^4 H^6 As^2 . S$	1st product of the action of sulphur.
$C^4 H^6 As^2 . S^2$	2nd ... ...
$C^4 H^6 As^2 . S^3$	3d ... ...

Bisulphuret of Kakodyle.

This compound is best obtained by means of the protosulphuret of kakodyle, which is formed by repeatedly distilling chloride of kakodyle with sulphuret of barium. 100 parts of the anhydrous sulphuret must be digested with 13·2 parts of dry sulphur until the whole is dissolved, and the white mass thus produced dissolved in æther; the crystals obtained from this solution are almost pure, they may be obtained quite so by adding a few drops of the protosulphuret and crystallizing from aqueous alcohol.

These crystals are large rhombic tables unchangeable by exposure to the air, and possess a most powerful and penetrating smell of assafœtida. Heated above 40° C., they fuse into a colourless liquid, which on cooling forms a radiated cry-

stalline mass. When heated more strongly it is decomposed, like many inorganic sulphurets, into free sulphur and protosulphuret of kakodyle, which is driven off. This compound is easily soluble in alcohol and æther, but perfectly insoluble in water; concentrated nitric acid oxidizes it with great violence; fuming acid even causes combustion and explosion. It may be reduced with the same ease with which it is formed. Mercury effects it in the cold; the products are sulphuret of mercury and protosulphuret of kakodyle. The products of this action will of course be different at different temperatures, for I have already shown that the protosulphuret is decomposed at a temperature of 200° C.

Persulphuret or Tersulphuret of Kakodyle.

I have not succeeded in obtaining this substance in an isolated state, but the action of sulphuretted hydrogen on kakodylic acid and its salts renders the existence of a sulphuret analogous to kakodylic acid almost a matter of certainty; this action of sulphuretted hydrogen is extremely remarkable. When dry sulphuretted hydrogen is passed over anhydrous kakodylic acid, even at ordinary temperatures, such an intense heat is produced that the vessel containing the acid must be well cooled in order to prevent a complete decomposition of the products: these are protosulphuret and bisulphuret of kakodyl and free sulphur. The same decomposition ensues when sulphuretted hydrogen is passed through an alcoholic solution of kakodylic acid. This action is perfectly similar to that of sulphuretted hydrogen on several metallic acids.

If however sulphuretted hydrogen be passed into a solution of kakodylate of potassa, there is no more precipitate than in the cases of the salts of metallic acids: this is only produced on the addition of a stronger acid, and is the same as that produced from kakodylic acid.

If however acetic acid be added to the solution until a feeble acid reaction is observable, no precipitation is produced. Solutions of metallic salts produce in this mixture precipitates similar to the kakodylates, in which the tersulphuret of kakodyle plays the part of the acid, and the metallic sulphuret that of the base.

I must for the present content myself with simply noticing the existence of these curious sulphur salts, at a future period I shall publish a fuller examination of them.

The results of the above research prove most satisfactorily the great similarity which exists between kakodyle and certain metals.

XXIX. *New Criteria for the Imaginary Roots of Equations. By* J. R. Young, *Esq., Professor of Mathematics in Belfast College**.

IT is shown in my recently published work on Equations of the higher orders, that Sturm's function, X_2, derived from the general equation

$$A_n x^n + A_{n-1} x^{n-1} + A_{n-2} x^{n-2} + \ldots A_2 x^2 + A_x + N = 0$$

is $$X_2 = \{(n-1) A^2_{n-1} - 2 n A_n A_{n-2}\} x^{n-2} + \&c.;$$

and it is known that if the leading coefficient here exhibited, that is the expression within the braces, be negative, the proposed equation must have one pair of imaginary roots, at least. Hence we have the criterion

$$2 n A_n A_{n-2} > (n-1) A^2_{n-1}, \ldots\ldots \quad [1.]$$

the satisfying of which will always imply the entrance of a pair of imaginary roots.

If the order of the coefficients of the proposed equation be reversed, we shall have a new equation, whose roots will be the reciprocals of the roots of the former equation. Hence the condition

$$2 n N A_2 > (n-1) A^2 \ldots\ldots\ldots \quad [2.]$$

will also imply the existence of a pair of imaginary roots. And it is plain, from the nature of Sturm's theorem, that in either of these criteria $>$ may be changed into $=$, whenever the proposed equation is above the second degree†; it is also obvious that the common criterion of imaginary roots, in quadratic equations, is only a particular case of the more general conditions [1.] and [2.].

It is worthy of notice, that when the condition [1.] has place, the imaginary roots, thus implied, can never be converted into real roots by means of any changes among the coefficients after the third: nor, when the condition [2.] has place, can any alteration in the coefficients which precede the last three terms convert the imaginary roots, thus implied, into real roots.

The criteria just exhibited, being very easily applied, will often save a good deal of labour in the analysis of equations. For example, the equation

$$x^5 - 36 x^3 + 72 x^2 - 37 x + 72 = 0$$

is immediately seen to satisfy the second criterion, and therefore to have a pair of imaginary roots. The partial analysis

* Communicated by the Author.

† As in the equation of the second degree, the roots in this case *may* all be equal: in order to which however *all* the coefficients of X_2 must be zero.

of the equation, by the method of Budan, leaves only one interval in doubt: the labour of examining this interval is thus spared.

The criteria exhibited above are only two of a group of $n-1$ criteria, arising from applying the second of those two to the several limiting equations derived from the primitive equation. By applying the formula [1.] to these derived equations, we shall merely obtain a repetition of the same inequality: for, as may be easily shown from the nature of differentiation, if a, b, c be the first three coefficients, either in the primitive or in any derived equation, m being the degree of that equation, the ratio

$$\frac{m\,a\,c}{(m-1)\,b^2}$$

will be constant. But if the formula [2.] be applied to the successive derived equations, we shall be led to the following distinct conditions; where, for uniformity, A_1 is put for A, and A_0 for N:

$$2\,n\,A_0\,A_2 > (n-1)\,A_1^2$$
$$3\,(n-1)\,A_1\,A_3 > 2\,(n-2)\,A_2^2$$
$$4\,(n-2)\,A_2\,A_4 > 3\,(n-3)\,A_3^2$$
$$5\,(n-3)\,A_3\,A_5 > 4\,(n-4)\,A_4^2$$
$$\vdots$$
$$n\,(n-[n-2])\,A_{n+2}\,A_n > (n-1)\,(n-[n-1])\,A^2_{n-1}$$

or $$2\,n\,A_{n-2}\,A_n > (n-1)\,A^2_{n-1}.$$

And if either of these conditions have place, we may infer the existence of imaginary roots in the proposed equation. Hence if we call any term in an equation, which lies between two terms with like signs, the *middle* term, we have the following general principle, viz.

If the product of the first and third of the three terms, multiplied by the exponent of the first and by n minus the exponent of the third, be greater than the square of the middle term multiplied by the exponent of that term and by n minus the same exponent, the equation must have imaginary roots. The well-known principle of De Gua is obviously included in this rule.

Each one of the series of inequalities given above involves three of the given coefficients; and, as in the cases at first considered, if either of these sets of three be preserved, it matters not how the remaining coefficients be altered: it follows therefore that the preceding conditions are perfectly inde-

pendent, so that equations may be framed that shall satisfy them all.

We cannot infer therefore that there are always as many pairs of imaginary roots as there are conditions fulfilled, although it is probable that there does exist a connexion between the number of conditions and the number of imaginary pairs. But this is a matter that requires further investigation.

Belfast, Jan. 13, 1843. [To be continued.]

XXX. *Notice of some new Minerals.* *By* THOMAS THOMSON, *M.D., F.R.S. L. & E., M.R.I.A., &c., Regius Professor of Chemistry in the University of Glasgow**.

ONE of the most abundant and important minerals is felspar, which constitutes the principal constituent in granite and gneiss, and together with hornblende forms the rocks so prevalent in this part of Scotland, I mean greenstone and basalt. Felspar is a double salt, being composed of 3 atoms of tersilicate of alumina and 1 atom of tersilicate of potash. Sometimes the potash is replaced by soda. The mineral in that case is distinguished by the name of *albite*, and differs in the shape of its crystals; three of the minerals which I mean to notice at present are connected with felspar, though they differ from it in their composition.

1. *Erythrite.*—The first species which I shall mention is *erythrite*. It occurs rather abundantly in the Kilpatrick hills, and also in the amygdaloid on the south side of the Clyde near Bishoptown. I do not know who first noticed it, but it was brought to me some years ago as a new mineral by Mr. Clackers, a mineral dealer in Old Kilpatrick. I call it *erythrite*, on account of the flesh-red colour which distinguishes all the specimens which I have seen.

Its specific gravity is about 2·541, which agrees with that of common felspar. And its hardness is about the same as that of felspar; the texture is compact, or at least not sensibly foliated, and I have never seen a specimen of it in crystals. It is composed of

Silica	67·90
Alumina	18·00
Peroxide of iron	2·70
Lime	1·00
Magnesia	3·25
Potash	7·50
Water	1·00
	101·35

* Read before the Glasgow Philosophical Society, 2nd of November 1842, and now communicated by the Author.

$$7\,(Al\,S^3) + Mg\,S^3 + K\,S^3$$
$$\text{Pyroxene } Cal\,S^2 + (\tfrac{2}{3}\,Mg + \tfrac{1}{3}f)\,S^2$$
$$\text{Amphibole } Cal\,S^3 + 3\,Mg\,S^2.$$

So that it differs from felspar by one-half of the potash being replaced by magnesia.

2. *Perthite.*—The next mineral I have to notice I distinguish by the name of *Perthite*. It was sent me by Mr. Wilson, a surgeon in Perth, a township of Upper Canada; hence the name by which I have distinguished it. It is very much connected with felspar in appearance, and was sent as a variety of that mineral.

The colour of the specimen sent me is white: it consists of a mass of crystals so united together as to form a kind of tesselated pavement. The crystals are obviously four-sided prisms, apparently rectangular, but not susceptible of measurement, because they cannot be isolated.

The lustre is vitreous; the hardness is rather less than that of felspar; but the specific gravity, which is 2·586, is identical with that of some of the varieties of that mineral. Its constituents were found to be

Silica	76·
Alumina	11·75
Magnesia	11·00
Protoxide of iron . .	0·225
Moisture	0·65
	99·625

From this analysis it is evident that it differs essentially from felspar; the quantity of silica is much greater, and the potash is entirely replaced by magnesia. Its constitution may be represented by the formula $6\,(Al\,S^4) + 5\,(Mg\,S^4)$. It is a quatersilicate, while felspar is a tersilicate. Could it be procured in sufficient quantity it would be an excellent material for the manufacture of porcelain.

3. *Peristerite**.—The next mineral which I have to mention was sent me also from Perth in Upper Canada, by Mr. Wilson, and also by Dr. Holmes of Montreal, under the name of *Iridescent felspar*; but neither its characters nor its composition correspond with that appellation.

The specimens were amorphous masses, and had the appearance of having constituted part of a rock blasted by gunpowder.

It is light brownish red, and exhibits a play of colours, chiefly blue, on the surface. It is translucent on the edges;

* From *περιστερά, a pigeon*, the colours resembling a pigeon's neck.

the lustre is vitreous and the texture imperfectly foliated: its hardness is only 3·75, which is a good deal less than that of felspar. Its specific gravity is 2·568.

Before the blowpipe it becomes white but does not melt. With carbonate of soda it melts into a green coloured bead, and on adding nitre the colour becomes red: with borax it fuses into a colourless bead.

Its constituents were found to be

Silica	72·35
Alumina	7·60
Potash	15·06
Lime	1·35
Magnesia	1·00
Oxides of iron and manganese	1·25
Moisture	0·50
	99·11

The silica is much greater than in felspar, and the alumina much less, while the proportion of potash is nearly the same. If we were to consider the lime and magnesia and the oxides of iron and manganese as accidental bodies united to silica in the same ratio as the alumina and the potash, the constitution of the mineral might be represented by $4\,(Al\,S^5) + 3\,(K\,S^5)$. If the lime and magnesia be essential constituents, the formula will be $Al\,S^5 + (\frac{6}{8}\,K + \frac{1}{8}\,Cal + \frac{1}{8}\,Mg)\,S^5$.

4. *Silicite.*—The fourth mineral which I shall notice I have distinguished by the name of *Silicite*, from the great resemblance which it has to quartz in its external aspect, though it differs entirely from that mineral in its constitution. It occurs in a basaltic rock in the county of Antrim, and was given me by Mr. Doran, an Irish mineral dealer.

The colour is white with a shade of yellow, the texture foliated, and the fracture small conchoidal. Its lustre is vitreous, its hardness nearly the same as that of quartz, and its specific gravity 2·666, or nearly the same as that of rock crystal.

With carbonate of soda it fuses into an opake bead, and with borax into a transparent colourless bead. Its constituents are

Silica	54·8
Alumina	28·4
Protoxide of iron	4·0
Lime	12·4
Water	0·64
	100·24

If we suppose the oxide of iron to be combined with alumina and to be only accidentally present, the constitution of silicite will be $7\,(Al\,S^2) + 2\,(Cal\,S)$.

It is a double anhydrous aluminous silicate. It differs from fuller's earth by containing 2 (Cal S) instead of 2 Aq.

5. *Gymnite.*—To the fifth mineral species which I mean to notice at present I have given the name of *Gymnite*, because its locality is the bare hills west of Baltimore. I got the specimen in my collection from Mr. Alger of Boston, well known for his and Mr. Jackson's excellent geological description of Nova Scotia.

The mineral was in amorphous pieces, having a very pale and dirty orange colour. It is translucent on the edges; the lustre is resinous. It is very tough and difficult to break: this makes it difficult to determine the hardness; but it is softer than felspar. The specific gravity is 2·2165. When held in the flame of a spirit-lamp it becomes dark brown: with soda it fuses into a white opake bead; with borax into a colourless bead; with nitrate of cobalt it assumes a rose-red colour.

Being subjected to analysis, its constituents were found to be

Silica	40·16
Magnesia	36·00
Water	21·60
Alumina with trace of iron . .	1·16
Lime	0·80
	99·72

It is therefore composed of silica, magnesia and water, and its constitution may be represented by the formula 2 (Mg S) + Mg S^2 + 4 Aq.

6. *Baltimorite.*—For the next mineral species which I mean to notice I am also indebted to Mr. Alger. The specimen was labelled *Asbestus with chrome*, and the locality Baltimore; on this account I have given the species the name of Baltimorite.

The colour is grayish-green. The mineral is composed of longitudinal fibres, adhering to each other, and has a considerable resemblance to asbestus; the lustre is silky. The mineral is opake; but when very thin it is translucent on the edges. It is a very little softer than calcareous spar. It does not fuse before the blowpipe, but assumes a brown colour. With soda melts into an opake, and with borax into a transparent bead. Its constituents are

Silica	40·95
Magnesia	34·70
Protoxide of iron	10·05
Alumina	1·50
Water	12·60
	99·8

Its constitution may be represented by the formula

$$14\ (Mg\ S) + 3\ (\tfrac{3}{4}f + \tfrac{1}{4}\ Al)\ S^2 + 11\ Aq.$$

Asbestus contains more silica and a good deal of lime, which is wanting in baltimorite: asbestus, in fact, is merely a variety of pyroxene.

7. For the next mineral, which from its constitution I call *subsesquisulphate of alumina*, I am also indebted to Mr. Alger. The locality is South Peru.

It is a soft, opake mineral, composed of silky fibres adhering to each other. The colour is white, but there is a reddish yellow tint which partially pervades the specimens, owing obviously to a little foreign matter with which they are stained. The taste is acid and sweet, like that of alum. The specific gravity is 1·584. It is soluble in water. The constituents are

Sulphuric acid	32·95
Alumina	22·55
Sulphate of soda	6·50
Water	39·20
	101·2

Obviously,	1 atom of sulphuric acid . . .	5·
	$1\frac{1}{2}$ atom of alumina	3·375
	1 atom of sulphate of soda . .	9·0
	5 atoms of water	5·625
		23·

The sulphate of soda exists in a greater proportion than sulphate of potash or of ammonia does in our alum. It is curious that in South America soda almost universally replaces the potash which occurs in other parts of the world. Instead of saltpetre, so abundant in India and even in Europe, we have nitrate of soda in Peru, and instead of potash alum, we find in Buenos Ayres and other districts of South America, soda alum deposited in amygdaloidal cavities in a kind of shale.

8. Messrs. Alger and Jackson gave the name of *Acadiolite* to a variety of chabasite which they found in Nova Scotia, and specimens of which Mr. Alger was kind enough to send to me. The colour of the mineral is yellow, and it has the crystalline shape and the characters of chabasite so completely, that it would be considered as a mere variety of that mineral were it not that the constituents do not quite agree. The specific gravity of acadiolite is 2·0202, and its constituents,

Silica	52·4
Alumina	12·4
Lime	11·6
Peroxide of iron	2·4
Water	21·6
	100·4

The proportion of alumina in chabasite is greater than in acadiolite. If this difference be constant acadiolite must be considered as a new species. Its constitution, and that of chabasite, may be represented by the formulas—

Acadiolite . . . $2\,(Al\,S^3) + Cal\,S^2 + 6\,Aq.$

Chabasite . . . $3\,(Al\,S^2) + Cal\,S^3 + 6\,Aq.$

9. *Prasilite.*—To the next mineral species which I shall mention I have given the name of *Prasilite*, from the green colour by which the only specimen which I have seen is characterized. It is found in the Kilpatrick hills, and was brought to me some years ago by a gentleman while attending my class. He had picked up the specimen and brought it that I might tell him its name. On looking at and examining its hardness and texture, I pronounced it to be sulphate of lime tinged by an admixture of epidote; but upon examining it chemically, I soon discovered that the opinion formed from its external character was erroneous.

The colour is dark leek-green, and the hardness not more than 1; for it does not scratch selenite. It is opake, and has a specific gravity of 2·311, which comes near to that of selenite. It may be crumbled to powder between the fingers. It is composed of fibres very loosely adhering together. When heated to redness it gives out 18 per cent. of water, assumes a light yellow colour, and becomes much harder. Being subjected to analysis, its constituents were found,—

Water	18·00
Silica	38·55
Magnesia	15·55
Lime	2·55
Peroxide of iron	14·90
Oxide of manganese . .	1·50
Alumina	5·65
	96·70

The loss, amounting to 3 per cent., was probably an alkali. Prasilite is obviously a triple sesquisilicate. Its constitution may be represented by the formula

$$8\,(Mg\,S^{1\frac{1}{2}}) + 4\,(f\,S^{1\frac{1}{2}}) + 3\,(Al\,S^{1\frac{1}{2}}) + 18\,Aq.$$

10. The next mineral which I shall notice is one which occurs in the beds of iron ore at Franklin in New Jersey, and was first noticed by Messrs. Keating and Vanuxem about the year 1822, under the name of Jeffersonite. Keating made an analysis of it, the result of which induced me to place it among the magnesian minerals, and intimately connected with pyroxene and amphibole. But having got a specimen of it through the kindness of Dr. Torrey of New York, I subjected

it to a new analysis. The result was so different from Mr. Keating's, that it became evident that the position which I had assigned it was a wrong one, and that in reality it was a quadruple salt consisting of silica united to the four bases, lime, alumina, iron, and magnesia.

The colour of Jeffersonite is dark olive green, passing into brown. It is foliated, and according to Keating, may be cleaved in various directions. The specimen in my possession is an imperfect four-sided prism; but the faces are not smooth enough to admit of measurement.

The lustre is resinous and almost semimetallic; the streak is gray, and the powder light green. It is rather harder than fluorspar, though softer than apatite. The specific gravity is 3·51. Before the blowpipe it fuses readily into a dark coloured globule: its constituents are

Silica	44·50
Lime	22·15
Alumina	14·55
Protoxide of iron	12·30
Magnesia	4·00
Moisture	1·85
	99·35

The constitution may be represented by the formula

$$4\,(\mathrm{Cal}\ \mathrm{S}) + 4\,(\mathrm{Al}\ \mathrm{S}) + 2\,(f\,\mathrm{S}^2) + \mathrm{Mg}\,\mathrm{S}^2\,;$$

so that it differs essentially in its composition from both pyroxene and amphibole.

XXXI. *On* Mr. Earnshaw's *Reply to the Defence of the Newtonian Law of Molecular Action. By the Rev.* P. Kelland, *M.A., F.R.SS. L. & E., &c., Professor of Mathematics in the University of Edinburgh**.

On receiving the Philosophical Magazine for December, 1842, in which Mr. Earnshaw terminates his Reply to me by requesting an answer to four questions, I thought it right not to delay complying with his request. But I am not, of course, hindered thereby from discussing the rest of his paper. In point of fact, I am in arrear two answers to Mr. Earnshaw, viz. to part of his paper in the November Number, and to that before me. I do not, however, see that the former of these requires any rejoinder. I admit the first and second remarks in it, understood of relative, not of absolute displacements, but do not see that they bear on the question before us. To the third remark I have already replied.

Let me then confine myself to the last paper, and examine

* Communicated by the Author.

the different portions of it in order. 1. I am told that an argument I have used "is considered as a strong indication of my having allowed other motives than a desire of truth to influence me in bringing it forward." It is quite true I had other motives,—motives of delicacy. I did not wish to make a conclusion so evidently at variance with the premises stand forth prominently in a friendly controversy. In the first sketch of my paper it bore a more conspicuous place than I afterwards permitted it to do. Mr. Earnshaw remarks that it stands in his Memoir as a purely casual observation. I am glad to learn he intends it as no more, and an incorrect one of course. It appeared to me to be the summing up of the argument which I was replying to; for this is the way in which it is introduced; "and *consequently* whether the particles are arranged in cubical forms, or in any other manner, there will always exist a direction of instability." (Art. 5). If I am to understand that Mr. Earnshaw withdraws this, then have I attained the main object of my reply to the objection from instability, for he then withdraws the arguments on which it is founded. I trust to the discernment of my readers to decide whether in what I said I stepped "out of the line of legitimate argument."

Mr. Earnshaw goes on to say, "unfortunately for the Professor, in this instance he reaps no advantage by stepping out of the line of legitimate argument, as his objection is founded on the misconception that I have supposed the particles to be in equilibrium." I reply that I certainly did consider that the portion of Mr. Earnshaw's Memoir which relates to *instability* admitted that the particles are (or at least *may be*) supposed in their position of rest. Had I conceived Mr. Earnshaw would not allow this, I certainly should not have thought his objections worth answering, and I apologize for having troubled my readers with a reply. But I must not on that account refuse Mr. Earnshaw's request (top of p. 438), "to point out the link of his argument against Newton's law which violates that supposition" (viz. that the particles are not in their position of rest). It is to be found in Art. 11 of his Memoir (Trans. Camb. Ph. Soc. v. 7), the enunciation of which is as follows:—"To find the force of restitution when a particle is slightly disturbed from its *position of equilibrium*." In this article it is assumed that $\frac{dV}{df} = 0$, or the *force* on the particle parallel to any axis is zero; the particle being in its position of rest, and the other particles in their positions *not* of rest. This assumption is manifestly incorrect. It amounts to the following: *By moving all the particles of a system but one in*

any manner whatever, no force is put in play on the one which is not moved. But Mr. Earnshaw refers to Art. 4 for proof. There is nothing about $\frac{dV}{df}$ in that article, so I suppose this is a misprint for Art. 3, where $\frac{dV}{df}$ is said to be equal to 0 when the position of the point is one of *neutral attraction*, i. e. when the force $\left(\frac{dV}{df}\right)$ *is* 0. But why it is also 0 when the point is in a position of *equilibrium* (a very different thing from neutral attraction when the other particles are not in their positions of rest), does not appear. If any one doubts whether $\frac{dV}{df}$, or the force parallel to an axis be really 0 or not, in such circumstances, I refer him to M. Cauchy's demonstration, that it is not in the *Nouveaux Exercices*, p. 190, or the *Exercices d'Analyse*, p. 304.

But that I am justified in my misconception (in supposing that Mr. Earnshaw's Memoir has reference to equilibrium), will, I hope, be admitted by any one who reads the hypothesis on which it is based. "It is assumed that the other consists of detached particles; each of which *is in a position of equilibrium*, and when slightly disturbed is capable of vibrating in any direction." Further, a point of neutral attraction and a position of equilibrium are used as synonymous, Arts. 3, 4, 6, 11, &c. And moreover, if equilibrium is a *failing case* in his objection (Art. 15), that "the equilibrium can only be stable in one plane," I am at a loss to know what the objection itself amounts to.

2. I turn now to the second portion of Mr. Earnshaw's Reply (p. 438). It is quite unconnected with the former. Relative to the first four paragraphs, I beg to direct attention to the previous objection of my opponent and to my Reply. The objection is this (Phil. Mag. for July, 1842, p. 47): the equation $2\pi^2\left(\frac{v}{\lambda}\right)^2 = \Sigma\left(A_r \sin^2\frac{rh}{2}\right)$ is such "that its right-hand member involves λ implicitly, in a manner which depends upon the arrangement of the molecules of æther, &c. Hence if there be dispersion in a medium on the finite interval theory, there must be dispersion *in vacuo* also." To this I replied by stating that this expression *in a medium* "must contain a term due to the action of the particles of matter." (Phil. Mag. for Oct. 1842, p. 264.) Mr. Earnshaw's argument assumes that it does not. Now in the Reply before me, it is attempted to be shown that the action of the particles of matter is included.

Clearly therefore the *form* of the function in this case, and where there are no particles of matter, is *different*, and Mr. Earnshaw withdraws his objection that they must be the same. I have only to add in answer to the suggestion, "Perhaps the Professor will point out what step of my investigation implies the existence of the absent particles:" *none whatever.* The symbol Σ may take in everything. And this puts the matter in the most simple light as regards Mr. Earnshaw's *inference.* If Σ in a medium is discontinuous, and *in vacuo* continuous, then have we the clearest reason why the expression *does* depend on h in the one, and *does not* in the other. The next paragraph has reference to another subject,—numerical verification. Of course no one considers an *error* of calculation as strengthening a theory. And I have already explained why the *processes* employed do so (p. 267).

3. The paragraph at the foot of p. 440, is a reiteration of Mr. Earnshaw's assertion that he has proved $v = 0$ or $n = 0$, &c. As this is of very great importance, the consequences being broadly hinted at by Mr. Earnshaw, I deem it requisite to state that I find three proofs of it. The first (Phil. Mag. for Nov., p. 341) depends on the assumption that v, v', and v'' are equal, which they are not. The second (Memoir, Art. 8), on the assumption that $\frac{d^2 \alpha}{d t^2} = \frac{d^2 \mathrm{V}}{d f^2} \alpha$, which it is not, as I have shown above. The third (Phil. Mag. for Jan. 1843, p. 24), on the assumption that an exponential function is inconsistent with the reductions effected by means of a circular one. To this I replied in my last*. The objection that $v = 0$ is so important that it ought not to be lightly passed over. If it is admitted, then a considerable portion of the writings of Cauchy and myself must be incorrect. No one I am sure will attach any weight to the arguments which I have mentioned, but lest any one should conceive the *possibility* of proving the function to be zero, I write it down,

$$n^2 = 2 \Sigma \left(\frac{1}{r^3} - \frac{3 \delta x^2}{r^5} \right) \sin^2 \frac{\pi \delta x}{\lambda}$$

taken throughout the whole medium. This expression can be summed so as to depend on a single definite integral with respect to r, viz.

$$n^2 = \Sigma \left(- \frac{\sin k r}{k r^4} + \frac{3 \sin k r}{k^3 r^6} - \frac{3 \cos k r}{k^2 r^5} \right)$$

where $k = \frac{2 \pi}{\lambda}$.

* I may add that it *assumes* the existence of transverse and normal vibrations at the same time.

The sum of this function will depend on the distance between two consecutive particles: when that distance is exceedingly small (ε) it is $\frac{\pi k^2}{45 \varepsilon}$ (abstracting from sign), as I shall have to prove in the prosecution of my arguments in reply to the two remaining objections to Newton's law of molecular action.

4. Mr. Earnshaw *explains* his equations which he asserts I have misunderstood. "I fear it will give to my letter an air *of great sameness* if I again accuse the Professor of misunderstanding what he attempts to criticise." The equations in their first form (Phil. Mag. for May, p. 373) are the same as Cauchy's well-known ones. But the coefficients, it appears, are very different. M. Cauchy's depend on the direction of transmission, Mr. Earnshaw's do not. This was the ground of my objection to the latter. Let us see the reply. "I ask how does the Professor know that these coefficients are not equal?" "Does it depend upon the direction of transmission? This question and a similar one for each of the other coefficients M. Cauchy has not answered, but I have answered it for myself in the negative, on experimental grounds."

It is quite true that Cauchy does not (in the Memoir alluded to) answer the question, for a most obvious reason. He could never have conceived it to admit of doubt. What is the problem they are solving? It is this: *To find the vibrations which are capable of being transmitted, when the position of the plane of the wave is given.* Had it turned out that the coefficients are independent of the position of the plane of the wave, one of two consequences must have followed; either,—1, that any vibrations whatever may be transmitted along a given direction or with a given wave; or 2, that only certain vibrations can be transmitted, whatever be the plane of the wave; both of which are contrary to experiment. I say then that Cauchy could not conceive it possible that his coefficients should be independent of the plane of the wave. But I add, that although he did not *give* their values, he left only one step to be supplied for their determination. The coefficients D, E, and F, are expressed at p. 38 (equations 70), viz.

$$D = 2\,R\,b\,c\,k^2,\ E = 2\,R\,a\,c\,k^2,\ F = 2\,R\,a\,b\,k^2,$$

$$\therefore D : E : F :: \frac{1}{a} : \frac{1}{b} : \frac{1}{c} :$$

that is, these coefficients are reciprocally proportional to the cosines of the angles which the perpendicular to the plane of the wave makes with the coordinate axes. It is evident therefore that they depend on the position of the plane of the wave. Since, then, Mr. Earnshaw's do *not*, are we to con-

clude with him, "that M. Cauchy's are at variance with experiment?" I believe very few persons will be found to join in this opinion. M. Cauchy's name, in the first place, is a sufficient guarantee for the accuracy of results which he has repeatedly obtained at different remote intervals. But, in the next place, the same problem has been solved, in different forms, by Mr. Airy (Tracts, Art. 110), by M. Naumann, by myself, by Mr. Green (Trans. Camb. Phil. Soc., 129), and lastly by Mr. O'Brien (Phil. Mag., March 1842, p. 210), all arriving at like results, viz. that the equation $\frac{d^2 \xi}{d t^2} = - n^2 \xi$, &c. correspond generally to three directions determined *relative to the front of the wave*, not to the axes of symmetry in the medium *only*, or in a medium of symmetry *at all*. [See Mr. O'Brien's paper, Phil. Mag., March 1842, p. 211.] But, lastly, Mr. Earnshaw says he effects his reductions on *experimental* grounds. On what kind of experiments? let me ask. In the next page (442) we find again, "The forms of the new equations of motion

$$\left(\frac{d^2 \xi}{d t^2} = - k_1{}^2 \xi, \quad \frac{d^2 y}{d t^2} = - k_2{}^2 \eta, \quad \frac{d^2 \zeta}{d t^2} = - k_2{}^2 \zeta\right)$$

show that *these axes* are *axes of dynamical symmetry*,—those in fact which are better known as the axes of elasticity. Now from *experiment* we know that $k_1{}^2$, $k_2{}^2$, k^3 are constant quantities, i. e. independent of the wave's front." The *inference* which Mr. Earnshaw here draws from his equations is directly opposed to that drawn by *all* the authors quoted above. Other writers consider their symmetry to refer to *the front of the wave*. But, not to waste words on an error so obvious, let me ask Mr. Earnshaw a question. Are $k_1{}^2$, $k_2{}^2$, $k_3{}^2$ equal or unequal in uncrystallized media? If they are not, on what does their inequality depend? If they *are* equal; then can it be shown that $D = 0$, $E = 0$, $F = 0$, and $A = B = C$, so that the transformation is no transformation at all. If Mr. Earnshaw will carefully examine this remark, he will be convinced, I am sure, that the problem he conceives himself to be engaged in is the following:—"To find those directions within any medium, in which if a particle be disturbed, the resultant of the forces acting on it will tend to move it back in the same line in which the displacement is produced." This problem has been solved by Fresnel (*Mém de l'Institut*, 1824), by Herschel (Light, Art. 1001), and by A. S. in the Cambridge Mathematical Journal, vol. i. p. 3. Now this is a totally different problem from that against which Mr. Earnshaw brings his conclusions to bear. In the latter we are not

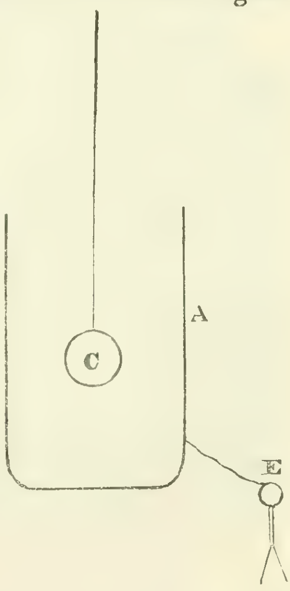
so much concerned with how a particle must be displaced relatively to the *medium*, as with how it must be displaced relatively to *the front of the wave*. And the confounding of these two is (as I said before) the cause of Mr. Earnshaw's difficulties and the explanation of the inapplicability of his objections.

XXXII. *On Static Electrical Inductive Action. By* Michael Faraday, *Esq., D.C.L., F.R.S.*

To R. Phillips, Esq., F.R.S.

Dear Phillips,

PERHAPS you may think the following experiments worth notice; their value consists in their power to give a very precise and decided idea to the mind respecting certain principles of inductive electrical action, which I find are by many accepted with a degree of doubt or obscurity that takes away much of their importance: they are the expression and proof of certain parts of my view of induction*. Let A in the diagram represent an insulated pewter ice-pail ten and a half inches high and seven inches diameter, connected by a wire with a delicate gold-leaf electrometer E, and let C be a round brass ball insulated by a dry thread of white silk, three or four feet in length, so as to remove the influence of the hand holding it from the ice-pail below. Let A be perfectly discharged, then let C be charged at a distance by a machine or Leyden jar, and introduced into A as in the figure. If C be positive, E also will diverge positively; if C be taken away, E will collapse perfectly, the apparatus being in good order. As C enters the vessel A the divergence of E will increase until C is about three inches below the edge of the vessel, and will remain quite steady and unchanged for any lower distance. This shows that at that distance the inductive ac-

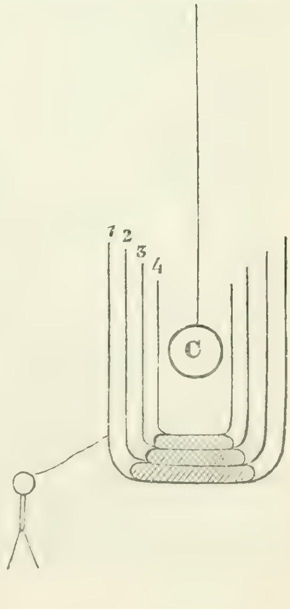


* See Experimental Researches, Par. 1295, &c., 1667, &c., and Answer to Dr. Hare, Philosophical Magazine, 1840, S. 3. vol. xvii. p. 56. viii.

tion of C is entirely exerted upon the interior of A, and not in any degree directly upon external objects. If C be made to touch the bottom of A, *all* its charge is communicated to A; there is no longer any inductive action between C and A, and C, upon being withdrawn and examined, is found perfectly discharged.

These are all well-known and recognised actions, but being a little varied, the following conclusions may be drawn from them. If C be merely suspended in A, it acts upon it by induction, evolving electricity of its own kind on the outside of A; but if C touch A its electricity is then communicated to it, and the electricity that is afterwards upon the outside of A may be considered as that which was originally upon the carrier C. As this change, however, produces no effect upon the leaves of the electrometer, it proves that the electricity *induced* by C and the electricity *in* C are accurately equal in amount and power.

Again, if C charged be held equidistant from the bottom and sides of A at one moment, and at another be held as close to the bottom as possible without discharging to A, still the divergence remains absolutely unchanged, showing that whether C acts at a considerable distance or at the very smallest distance, the amount of its force is the same. So also if it be held excentric and near to the side of the ice-pail in one place, so as to make the inductive action take place in lines expressing almost every degree of force in different directions, still the sum of their forces is the same constant quantity as that obtained before; for the leaves alter not. Nothing like expansion or coercion of the electric force appears under these varying circumstances.



I can now describe experiments with many concentric metallic vessels arranged as in the diagram, where four ice-pails are represented insulated from each other by plates of shell-lac on which they respectively stand. With this system the charged carrier C acts precisely as with the single vessel, so that the intervention of many conducting plates causes no difference in the amount of inductive

effect. If C touch the inside of vessel 4, still the leaves are unchanged. If 4 be taken out by a silk thread, the leaves perfectly collapse; if it be introduced again, they open out to the same degree as before. If 4 and 3 be connected by a wire let down between them by a silk thread, the leaves remain the same, and so they still remain if 3 and 2 be connected by a similar wire; yet all the electricity originally on the carrier and acting at a considerable distance, is now on the outside of 2, and acting through only a small non-conducting space. If at last it be communicated to the outside of 1, still the leaves remain unchanged.

Again, consider the charged carrier C in the centre of the system, the divergence of the electrometer measures its inductive influence; this divergence remains the same whether 1 be there alone, or whether all four vessels be there; whether these vessels be separate as to insulation, or whether 2, 3 and 4 be connected so as to represent a very thick metallic vessel, or whether all four vessels be connected.

Again, if in place of the metallic vessels 2, 3, 4, a thick vessel of shell-lac or of sulphur be introduced, or if any other variation in the character of the substance within the vessel 1 be made, still not the slightest change is by that caused upon the divergence of the leaves.

If in place of one carrier many carriers in different positions are within the inner vessel, there is no interference of one with the other; they act with the same amount of force outwardly as if the electricity were spread uniformly over one carrier, however much the distribution on each carrier may be disturbed by its neighbours. If the charge of one carrier be by contact given to vessel 4 and distributed over it, still the others act through and across it with the same final amount of force; and no state of charge given to any of the vessels 1, 2, 3, or 4, prevents a charged carrier introduced within 4 acting with precisely the same amount of force as if they were uncharged. If pieces of shell-lac, slung by white silk thread and excited, be introduced into the vessel, they act exactly as the metallic carriers, except that their charge cannot be communicated by contact to the metallic vessels.

Thus a certain amount of electricity acting within the centre of the vessel A exerts exactly the same power externally, whether it act by induction through the space between it and A, or whether it be transferred by conduction to A, so as absolutely to destroy the previous induction within. Also, as to the inductive action, whether the space between C and A be filled with air, or with shell-lac or sulphur, having above twice the specific inductive capacity of air; or contain many con-

centric shells of conducting matter; or be nine-tenths filled with conducting matter, or be metal on one side and shell-lac on the other; or whatever other means be taken to vary the forces, either by variation of distance or substance, or actual charge of the matter in this space, still the amount of action is precisely the same.

Hence if a body be charged, whether it be a particle or a mass, there is nothing about its action which can at all consist with the idea of exaltation or extinction; the amount of force is perfectly definite and unchangeable: or to those who in their minds represent the idea of the electric force by a fluid, there ought to be no notion of the compression or condensation of this fluid within itself, or of its coercibility, as some understand that phrase. The only mode of affecting this force is by connecting it with force of the same kind, either in the same or the contrary direction. If we oppose to it force of the contrary kind, we may *by discharge* neutralize the original force, or we may *without discharge* connect them by the simple laws and principles of static induction; but away from induction, which is *always of the same kind*, there is no other state of the power in a charged body; that is, there is no state of static electric force corresponding to the terms of *simulated* or *disguised* or *latent* electricity away from the ordinary principles of inductive action; nor is there any case where the electricity is *more latent* or *more disguised* than when it exists upon the charged conductor of an electrical machine and is ready to give a powerful spark to any body brought near it.

A curious consideration arises from this perfection of inductive action. Suppose a thin uncharged metallic globe two or three feet in diameter, insulated in the middle of a chamber, and then suppose the space within this globe occupied by myriads of little vesicles or particles charged alike with electricity (or differently), but each insulated from its neighbour and the globe; their inductive power would be such that the outside of the globe would be charged with a force equal to the sum of *all* their forces, and any part of this globe (not charged of itself) would give as long and powerful a spark to a body brought near it as if the electricity of all the particles near and distant were on the surface of the globe itself. If we pass from this consideration to the case of a cloud, then, though we cannot altogether compare the external surface of the cloud to the metallic surface of the globe, yet the previous inductive effects upon the *earth* and its buildings are the same; and when a charged cloud is over the earth, although its elec-

tricity may be diffused over every one of its particles, and no important part of the *inductric* charge be accumulated upon its under surface, yet the induction upon the earth will be as strong as if all that portion of force which is directed towards the earth *were* upon that surface; and the state of the earth and its tendency to discharge to the cloud will also be as strong in the former as in the latter case. As to whether lightning-discharge begins first at the cloud or at the earth, that is a matter far more difficult to decide than is usually supposed *; theoretical notions would lead me to expect that in most cases, perhaps in all, it begins at the earth. I am,

My dear Phillips, ever yours,

M. FARADAY.

Royal Institution,
4th Feb. 1843.

XXXIII. *On the Electrical Origin of Chemical Heat.* *By* JAMES P. JOULE, *Esq.*†

IN a paper ‡ which I read on the 2nd of last November before the Literary and Philosophical Society of this town, I endeavoured to account for the heat evolved by the combustion of certain bodies, on the hypothesis of its arising from resistance to the conduction of electricity between oxygen and the combustibles at the moment of their union. Taking this view of phænomena, I showed that the heat evolved by the union of two atoms is proportional to the electromotive force of the current passing between them, in other words, to the intensity of their chemical affinity.

In that paper I gave the results of my own experiments, and I apprehended that my numbers were below the truth on account of the simplicity of my apparatus. On comparing them, however, with the experiments of Dulong, which were conducted in a manner very well calculated to prevent loss of heat, I now find that they agree so well with the results of that very accurate philosopher as to show that the method I adopted of carrying on the combustion in the inner of two glass jars, while the heat evolved was measured by water placed between them, was not unworthy of reliance. In the following table I give the results of Dulong's experiments reduced to degrees Fahrenheit acquired by a pound of water.

* Experimental Researches, Par. 1370, 1410, 1484.

† Read before the British Association at Manchester, 25th June 1842; and now communicated by the Author.

‡ Published in the Phil. Mag. S. 3. vol. xx. p. 98.

Quantities converted into Protoxides.	Dulong's Results.	My own Experiments.	Theoret. Results.	Corrected Theoretical Results.
40 grs. of Potassium...	....	17·6	21·47	
33 grs. of Zinc...	10·98	11·03	13·83	11·01
28 grs. of Iron	9·00	9·48	12·36	8·06
31·6 grs. of Copper ...	5·18	....	9·97	5·97
1 gr. of Hydrogen ...	8·98	8·36	10·47	10·40*

In the above table there is one metal, copper, of which I did not treat in my former paper; it will therefore be well to explain the manner in which the theoretical results for it were obtained.

Platinum wires were immersed in a saturated solution of the sulphate of oxide of copper. These were successively connected with the poles of voltaic arrangements consisting of various numbers of Smee's pairs in series. Using two pairs, I had neither current nor decomposition. But with three there were electrolytic effects, oxygen being evolved from the positive, and copper being deposited on the negative electrode. The ratio of current passed by three and four pairs was as nearly as possible 1 : 4. Therefore $2\frac{2}{3}$ pairs are equal to the resistance to electrolysis of sulphate of oxide of copper.

Now if I calculate, as I did in my former paper for zinc, iron, and hydrogen, I must argue that electricity equal in intensity to that of $2\frac{2}{3}$ pairs passes between oxygen and copper when they unite by combustion. But one pair of Smee's battery can produce electricity of such intensity that a degree† of it will evolve 3°·74 of heat, and multiplying by $2\frac{2}{3}$ we have 9°·97, the quantity of heat which is evolved by a degree of electricity of $2\frac{2}{3}$ times that intensity; 9°·97 is therefore the theoretical result, if we suppose that the intensity required to overcome the resistance to electrolysis of sulphate of oxide of copper is equal to the intensity of current arising from the union of oxygen and copper in combustion.

There is, however, since the experiments of Daniell, reason to think that this is not the case, but that part of the *intensity* of a current engaged in electrolyzing these compound bodies, is used in separating the acid from the base prior to or (according to that philosopher's view) simultaneously with the decomposition of the latter. Unfortunately we cannot bring forward a direct experiment in proof of this fact, inasmuch as

* I now find that Prof. Daniell has proved the remarkable fact, that during the electrolysis of dilute sulphuric acid one quarter of an equivalent of acid goes along with the oxygen to the positive electrode. According to this the corrected theoretical result is 10·47; one quarter of the heat evolved by the union of water and sulphuric acid equal about 9·47.

† My degree of electricity is the quantity necessary to electrolyze an equivalent expressed in grains, as nine grains of water, &c.

the oxides* are by themselves, and at common temperatures, non-conductors of voltaic electricity, and therefore refuse to yield up their elements. But if on the principles of the theory we argue that the heat evolved on the combination of one equivalent with another is a measure of the intensity of the electric current passing between them at the time, we shall have the means of eliminating the electromotive force employed otherwise than in separating the elements of the oxides.

I suppose that there are three forces in operation, of which two are against, and one is for, a current engaged in electrolyzing the solution of the sulphate of a metallic oxide. The first two are the affinity of the elements of the oxide, and that of the oxide for sulphuric acid; and the third, which is in a contrary direction to the two others and generally less than either, is the affinity of water for sulphuric acid. We eliminate the two latter forces as follows:—

1st. For *Zinc*.—I find that 41 grs., or an equivalent of oxide of zinc, evolves 2°·82 when dissolved in dilute sulphuric acid. This, which is the quantity of heat due to the intensity of current resulting from the difference of the affinities of sulphuric acid for the oxides of zinc and hydrogen, leaves, when subtracted from 13°·83, 11°·01, the corrected theoretical result, which I have given in the 4th column of the table.

2nd. For *Iron*.—The black oxide is dissolved with such difficulty by dilute sulphuric acid that the heat thereby evolved cannot be accurately measured. However, the dissolution of the hydrate is easily effected, the quantity of heat generated thereby being, per equivalent, 2°·74. But we probably arrive nearer the truth by subtracting from the heat evolved by the dissolution of iron in dilute sulphuric acid, that portion which is due to the oxidation of the iron. In this way I have 5°·2 − 0°·9 = 4°·3, the quantity due to the solution of protoxide of iron in dilute sulphuric acid. This, when subtracted from 12°·36, leaves 8°·06, the corrected result of theory.

3rd. For *Copper*.—The protoxide of copper does not dissolve readily in dilute sulphuric acid. Nevertheless by keeping the temperature of the surrounding atmosphere equal to that of the liquid, I obtained, per equivalent of oxide, 4°·0, a result which may, I think, be relied on. Subtracting this from 9°·97, we obtain 5°·97.

4th. For *Hydrogen*, little correction is needed†. The liquid used in the experiments made to ascertain the resistance to electrolysis of water was mixed with a small quantity only

* I find that pure water is not at all decomposed by ten pairs of Smee's battery in series, the current being thereby almost if not quite cut off.

† On this question I now refer to note * appended to page 205.

of sulphuric acid. Consequently there were plenty of atoms of water either uncombined, or only slightly attached to the acid, prepared to give up their elements to the current with little or no additional resistance in consequence of its presence.

By inspecting the table it will be seen that these corrected theoretical results agree very well with the experiments of Dulong and myself. They accord accurately in the case of zinc. Iron gives results which are not equally satisfactory. But we must remember that it is converted by combustion into the magnetic oxide, and that a correction ought therefore to be applied on account of heat evolved by the union of protoxide with oxygen, which it is very difficult to prevent entirely. Potassium gives theoretical and experimental results as nearly alike as can, I think, be expected, considering the complicated process* by which the former were obtained, and the practical difficulty of the latter. In the case of hydrogen we might have anticipated that theory would exceed experiment; for the resistance to electrolysis of water appears generally greater than it really is, on account of the peculiar state which the platinum evolving hydrogen is apt to assume, which has, of course, the effect of increasing the theoretical value.

Besides the corrections to the theoretical results which I have supplied, I thought that there might be a slight one needed on account of *light* which is evolved in such abundance in some instances of combustion. It was of importance to ascertain whether in the evolution of light an equivalent of heat was absorbed. With this view I have made an extensive series of experiments with the voltaic apparatus, comparing the heat evolved when no light was exhibited, with that evolved when the conducting wire was ignited to whiteness. The mean of twelve experiments showed that the heat evolved by a certain quantity of wire immersed in water is, for a given quantity of current and a given length of time, $24^{\circ}\cdot75$; and the mean of sixteen experiments, in which a platinum wire inclosed in a glass tube surrounded with water was ignited so as to give out a quantity of light equal to that arising from a common tallow candle, gave $24^{\circ}\cdot4$ as the quantity of heat due in this latter case to similar circumstances of resistance and quantity of current. These experiments seem to indicate that heat is lost when light is evolved, but in so slight a degree that my experiments on the heat of combustion need not be corrected for it. Dulong's experiments were performed in a box of copper, which being opake would entirely obviate this source of error.

I conceive that the correctness of the idea, entertained, I

* Phil. Mag. S. 3. vol. xx. p. 109 (49).

believe, by Davy, and afterwards more explicitly mentioned by Berzelius, that the heat of combustion is an electrical phænomenon, is now rendered sufficiently evident. We have also shown that the heat arises from resistance to the conduction of electricity between the atoms of combustibles and oxygen at the moment of their union. Of the nature of this resistance we are still ignorant.

Some time ago I commenced an investigation on the heat arising from the union of sulphuric acid with potash, soda, and ammonia. This inquiry is more difficult than I expected, and my experiments are not yet sufficiently complete to lay before the British Association. In a future paper I hope to extend my inquiry, and also to show the relation of latent heat to electrical intensity.

XXXIV. *Experiments in Electricity.*

By Sir Graves C. Haughton, *K.H.*, *F.R.S.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

As every phænomenon connected with electricity must be interesting to the scientific world, whether it brings to light a new principle or merely confirms one that has been already established, I have thought that the following experiments might prove acceptable to your readers.

If a needle of any of the malleable metals, or of other substances, such as wood, ivory, quill and straw, or even of glass or sealing-wax, be placed in a galvanometer of the simplest form, and one end of the wire be fixed in *metallic* contact to the hook of the prime conductor of an electrifying machine, less than a quarter of a revolution of the handle will, if the machine be in good working order, cause the needle to stand at an angle of 90°. If the proper needle of the galvanometer be employed instead of one of the foregoing, it will place itself at an angle of from 75° to 80°, and will preserve that position as long as the machine is kept in operation. The needles with which these experiments were tried varied in length from three inches to five-eighths of an inch. The galvanometer stood at first simply on a mahogany table, but the results were not certain with every kind of needle, owing apparently to their faulty construction; when, however, it was placed on a good insulator the experiments never failed. It is worthy of remark, that whenever the state of the atmosphere was unfavourable to the working of the machine, which has been uniformly the case during the present month, in consequence of

the dampness of the weather, a singular difference occurred between two needles, one of which was of brass and the other magnetic, and both of them five-eighths of an inch in length. The brass one invariably placed itself at right angles, while the magnet remained uninfluenced, though it was the more delicate of the two, as it had an agate socket. This fact, as well as what I have just stated of the other magnetic needle, showed that there was a struggle between the polarity of the needle and the influence of the electric current. It was observed that whenever there was a powerful stream of electricity employed, it was seen to escape in a beautiful pencil of light from the point of the other end of the wire which was kept coiled up and unemployed. The machine used in these experiments is of the plate construction and eighteen inches in diameter, but is at present in very indifferent working order.

M. Becquerel mentions in his History of Electricity that he thought he had made needles of various substances move in a galvanometer when under the influence of a *voltaic* current, but that he afterwards found that the slight movements he had observed were owing to currents of air occasioned by the heat evolved. The present experiments, however, are so decisive and unequivocal that they cannot be attributed to such a cause; still my own convictions are, that they are not dependent upon magnetic influence.

I am, Sir, &c.,

January 31, 1843. GRAVES C. HAUGHTON.

XXXV. *On the Biniodide of Mercury. By* ROBERT WARINGTON, *Esq., Secretary to the Chemical Society, &c.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I SHALL feel obliged by your inserting in an early Number of your Journal the following observations in reply to a letter on this subject by Mr. Fox Talbot, which appeared in the Number of the Philosophical Magazine for November 1842 (page 336); at the same time I cannot help regretting that your pages should be occupied by a matter of personal discussion; but as it would appear, from the contents of the letter in question, that I had, either ignorantly or wilfully, made use of the published observations of another, and as Mr. Talbot pointedly "begs to draw my attention" to the question of *his priority to the* "*discovery* of one of the most curious phænomena in optics," it obliges me to adopt the same public channel of reply for the purpose of clearing my-

self from the implications contained in his letter. Had my time permitted I should have replied to this charge at an earlier date, but professional engagements have rendered this delay unavoidable.

In the communication read before the Chemical Society on February 1, 1842, and afterwards inserted in your Journal of September of the same year, I do not lay any claim to the discovery of many of the phænomena therein detailed; nay more, it is distinctly stated (p. 193), "The resumption of the scarlet colour (in the sublimed yellow crystals of the biniodide) *has been attributed to an alteration in the molecular arrangement of the crystals, and it was with the view of clearly ascertaining this point that the following microscopic investigations were undertaken.*" I am at a loss to imagine how this can be construed into any wish on my part to deprive Mr. Talbot of what he claims as his discovery. But we must proceed to examine this matter with more care. In 1830 the alleged discovery of Mr. Talbot of 1836 was shown me by Mr. J. T. Cooper, with whom I was then assistant, and in the same year the phænomena claimed were exhibited as class-room illustrations; however, we must go even further back to the original paper on the subject by M. A. Hayes, and which was published in the American Journal of Science and Arts for 1829, vol. xvi. p. 174. This paper was republished in the Quarterly Journal of Science of the Royal Institution for 1830, p. 208; and it was noticed in the Paris edition of Berzelius's *Traité de Chémie* of 1830. Irrespective of these facts, the observations made by M. A. Hayes are so exactly those claimed by Mr. Talbot in his November paper, that I feel bound to place them in juxtaposition.

Mr. Fox Talbot, 1836.

"In that memoir (Phil. Mag., 1836, S. 3. vol. ix. p. 1.) I have shown,—

"1st. That when iodide of mercury is sublimed between two plates of glass nearly in contact with each other, it cools in the form of thin rhombic plates of a pale yellow colour.

"2nd. These often retain their colour when cold, if left undisturbed.

"3rd. But if such a crystal is disturbed, as for example, by touching it with a needle at any

M. A. Hayes, 1829.

"If the precipitate, obtained as above, by the addition of iodide of iron in solution to one of bichloride of mercury, be heated in a small subliming apparatus, or in a glass tube, it melts and sublimes copiously, and the vapour is condensed in large transparent rhombic plates, of a fine sulphur yellow colour.

"These crystals are permanent in the air and unaltered by the direct solar rays; but the slightest friction or the contact of a fine point, is sufficient to alter their interior arrangement. The point

point of its surface, it instantly turns scarlet at the point touched, and the scarlet colour is rapidly propagated over the whole crystal.

"4th. The crystal moves, and is spontaneously agitated during the time it is changing colour.

"6th. I added that I thought this phænomenon *the most evident proof we yet possessed of the dependency of colour upon internal molecular arrangement.*"

of contact instantly becomes of a rich scarlet, and the same colour spreads over the whole surface of a single crystal, and extends to the most remote angle, if a group of crystals be the subject of experiment.

"This change of colour is accompanied by a sensible mechanical motion, so that a small heap of the crystals appears as if animated, affording an elegant illustration of the connexion between colour and the mechanical structure of bodies."

A comparison of the above extracts will doubtless entirely alter Mr. Talbot's opinion as to these points of his claims to originality in regard to the observations of 1836.

I must now turn to the printed paper in the 9th vol. (S. 3.) of the Philosophical Magazine, p. 2. Mr. Talbot there admits "that chemical writers have observed" these "changes of colour," quotes Dr. Inglis's authority for the retention of the yellow tint for a considerable time, and states, that "wishing to examine into the cause of these facts" he instituted certain experiments, which are then detailed; so, that independent of the above paper in 1829, of M. A. Hayes, Mr. Talbot proves in this very paper of 1836, that Nos. 1, 2, and 3 of the points claimed in 1842 to have been "sufficiently established" by him in 1836 had been previously observed. The next point has reference to the first part of the claim No. 6. In 1836 Mr. Talbot states that "the change of colour is accompanied by a visible internal motion in the crystal, like a sinking or giving way of *successive ranks of particles.*" Here is not a syllable about *laminæ* which he has now laid claim to, and which term I consider implies a very different effect from either "ranks of particles" or "rows of molecules:" it is used in my paper to represent the plates of the crystal. Again, as to the commencement of the change of colour taking place "by the appearance of a red streak along one of its sides or edges," and "the boundary of the red and yellow" being "a straight line *parallel to two sides of the rhomboid,*" and that "its motion is across the crystal from one of these sides to the opposite one;" on these points I cannot agree with Mr. Talbot; and he will perceive in A, fig. 1, *c*, *d* and *e*, fig. 2, of my own paper, that the very reverse is shown, by drawings taken directly from the field of the microscope by the camera lucida; so that

his claim No. 5 is incorrect. With respect to the claim No. 7, my observations regarding the effects of polarized light apply to the last set of experiments on the precipitated iodide while suspended in the mixed solutions, and therefore Mr. Talbot's remarks, referring as they do to the sublimed crystals, cannot in the slightest degree interfere with them. As to the quoted expression, "the field of view appears scattered with the most brilliant assemblages of (rubies, topazes, emeralds and other) highly coloured gems," &c., the names of which, inclosed in the parenthesis, have, inadvertently I presume, been omitted in the quotation, the only similar word that I can find which occurs in my own description of these appearances is the word 'gems,' and if the use of that expression, applied as it is to the precipitated biniodide of mercury, is to be considered as implying a piracy on Mr. Talbot's application of it in viewing the crystals of sulphate of copper by polarized light, I can only plead the poverty of the English language, as I had never seen Mr. Talbot's paper (Phil. Mag. vol. v. p. 324) until he publicly drew my attention to it.

In the concluding paragraph of Mr. Talbot's letter he first states that "the second part of my paper, however, contains a fact both new and important," and then immediately afterwards *proves* that it is not new at all, for that he described in 1836 a phænomenon of the same kind previously unexampled, and that this is only a second example. Now surely these statements are perfectly contradictory. Observed facts cannot be new and old. But let us examine in what respects these phænomena are alike. Mr. Talbot takes iodide of lead, prepared through the medium of the acetate of that metal and iodide of potassium, while fresh, he does not say whether it was washed to remove the acetate of potash or not, and warms it over a spirit-lamp, when certain beautiful changes are observed. My own observations are on the precipitated biniodide of mercury, which is known to be first precipitated yellow, then gradually to deepen in tint, and ultimately to become scarlet; under the microscope the first precipitate is in minute crystals of the rhombic form, similar to the yellow sublimed salt, and these slowly and *spontaneously* become disintegrated, dissolve and are replaced by the octohedron with the square base like those obtained by the more cautious sublimation. I am at a loss to imagine where the similarity between them exists. It would take up too much space to go further into this matter, and I must content myself with leaving it to the study of any of your readers who may wish further to analyse this subject. Again apologizing for thus occupying so much room in your Journal, I remain, Gentlemen, Yours, &c.,

Apothecaries' Hall,
Feb. 10, 1843.

ROBERT WARINGTON.

XXXVI. *On the Cause of the Colours in Iridescent Agate.* By Sir David Brewster, *K.H., D.C.L., F.R.S., & V.P.R.S. Edin.**

In the Philosophical Transactions for 1813 †, I have described the general phænomena of colours produced in iridescent agates of different kinds, but I was not able to discover the cause by which these colours were produced. The spectra which accompanied the colourless image of a luminous body seen through the agate had a decided resemblance to those produced by diffraction in grooved surfaces, but as no grooves existed on the surface of the agate, as in mother-of-pearl, and as no veins could be seen in the interior of the mineral by the most powerful microscope which I then possessed, I was not entitled to ascribe the colours to an invisible agency. In a subsequent examination ‡ of the coloured images produced by certain specimens of calcareous spar, inclosing oppositely crystallized veins, I was led to suppose, from the observation of some similarly placed veins in particular specimens of agate, that the colours were those of polarized light as in calcareous spar; but re-examination of the phænomena in new specimens of agate afterwards convinced me that this opinion was not correct.

In repeating all my early experiments, with a little more experience and knowledge of the subject, I soon perceived that the actual phænomena were identical with those of the diffraction spectra. The coloured spectra in the agate suffered no change by increasing or diminishing the thickness of the plate. The less refrangible half of the spectrum was greatly expanded, and, in some good specimens, I observed the repetition of the spectra *three* times at equal intervals, and with increasing dispersion. In the specimen represented in plate v. fig. 1. of the Philosophical Transactions for 1814 §, I had observed that the *second* spectrum was only a little further distant from the colourless image than the *first* spectrum, which was 28° distant from that image. This fact, as it then appeared to me, put an end to the supposition that two such consecutive spectra could be produced by diffraction; but upon re-examining this specimen, I find that, though my observation of the fact was correct, yet I was wrong in considering it as a *second* spectrum connected with the *first* spectrum of 28°. It was, in reality, a *first* spectrum distant 31° from

* Communicated by the Author.

† Phil. Trans. 1813, p. 102, 103; 1814, p. 197–199.

‡ Ibid. 1815, p. 287.

§ Ibid. 1814, p. 198, par. 2. [or Phil. Mag. S. 1. vol. xlii. p. 286, 287; xliv. p. 267, 268.]

the colourless image, and produced by a part of the stripe of agate which had a different structure from the part of it which gave the spectrum of 28° *.

Having removed this difficulty I submitted a variety of agates to the microscope, and I found some which gave very faint diffraction spectra exceedingly close to the colourless image, and in those cases I could distinctly see the edges of the thin veins of which that part of the agate was composed, the number of these veins in an inch corresponding with the distance of the diffraction spectra. This result encouraged me to examine the beautiful specimen represented in fig. 2. plate v. of the paper already referred to, in which the part that produced the spectra exhibited no other difference from the part which did not produce them, than that of having a coarser rippled structure. I employed for this purpose a very fine achromatic microscope made by Messrs. A. Ross and Co., and after an accurate adjustment of the illuminating apparatus, I succeeded in discovering that the whole portion of the agate which produced the prismatic spectra consisted of veins so exceedingly minute that *seventeen thousand* of them would be required to make an inch. If, using Fraunhofer's letters, we call the thickness of a vein δ, the interval between the veins γ, and $\delta + \gamma = \varepsilon$, then $\varepsilon = \frac{1}{8610}$ of an inch; and as δ is nearly equal to γ we have $\delta = \frac{1}{17220}$dths part of an inch. In other specimens I have obtained the following results:—

Values of ε.	Thickness of each vein or δ.
$\frac{1}{8610}$ of an inch.	$\frac{1}{17220}$ of an inch.
$\frac{1}{11070}$	$\frac{1}{22140}$
$\frac{1}{22960}$	$\frac{1}{45920}$
$\frac{1}{25420}$	$\frac{1}{50840}$
$\frac{1}{27880}$	$\frac{1}{55760}$

As it is only in the first of these specimens that I have yet been able to discover the veined structure, we may consider these iridescent agates, when cut into thin plates, so that the veins are perpendicular to the two parallel faces, as affording the most difficult tests in microscopical observations.

In diffraction experiments this property of the agate may

* In fig. 1, plate v. of the Phil. Trans. 1814, A B is the stripe which produced these two spectra, the one of 28° being produced by the part *m o p n*, and the other of 31° by the part *o w x p*.

be found very useful. In one specimen which I have examined, having its faces inclined 2° or 3° to each other, I can distinctly see the line D of the spectrum formed from the light of a candle with a salted wick; and I have no doubt that specimens of agate will be found, and may be so nicely prepared in extremely thin plates, with their surfaces perpendicular to the veins, as to give diffraction spectra more perfect, and much more enlarged than it is possible to obtain from any system of parallel grooves that can be produced by art.

To the mineralogist this determination of the structure of the agate cannot fail to be interesting. The difference in the colour of the veins and their intervals, and their singular equality of thickness, is very remarkable. In the structure of mother-of-pearl, the succession of strata or veins marks the period of rest during which the animal has ceased to labour; and in the structure of *nacrite*, the artificial mother-of-pearl formed upon the dash-wheel at the cotton-works at Catrine*, the passage of one stratum into another indicates the daily rest of the wheel, and of the operations to which it gives rise; but it is not easy to understand how an aqueous solution of silex contained in the cavity of a solid rock, should deposit its solid contents with that uniformity and regularity which are found in structures depending on the periods of animal life or human labour.

St. Leonard's College, St. Andrew's,
February 13, 1843.

XXXVII. *On the Geology of Egypt.* *By* Lieut. NEWBOLD, *F.R.S., of the Madras Army*†.

MR. Newbold first describes the physical features of Egypt, and 2ndly, the formations of which the country is composed.

I. *Physical Features.*—After alluding to the natural boundaries of Egypt, namely, the Mediterranean on the north, the Libyan desert on the west, the mountains of Nubia on the south, and the Red Sea, with the Isthmus of Suez, on the east, and stating that the area thus circumscribed comprises about 100,000 square miles, the author shows that Egypt has three great physical divisions: 1. the mountainous region extending between the Red Sea and the Nile; 2. the deserts east and west of the Nile; and 3. the fertile valley of that river, with its delta.

The mountainous region is naked and dreary in aspect, on account of the deficiency of springs, rain, and dew; and it presents bare or

* Phil. Trans., 1836, p. 49, and this Journal, S. 3. vol. x. p. 201.

† From the Proceedings of the Geological Society, vol. iii. part 2, being an abstract of a paper read June 29, 1842.

sand-covered rocks, intersected by deep ravines. The peculiarly tabular features of Central and part of Upper Egypt are due to the horizontal stratification of the prevailing limestone, which supports the desert districts, and terminates near the banks of the Nile, from Esneh to Cairo, in mural escarpments. Between Kossier and Ghennah the aspect is rendered more varied and irregular by pinnacles and dome-shaped masses of plutonic or hypogene rocks. The deserts present a series of undulating plains sometimes studded with clusters of low hills, and are covered chiefly with unproductive saline, often calcareous and gypseous sand, marl, and gravel. The Oases of the deserts and the mountainous region, Mr. Newbold regards simply as valleys, supplied with moisture either by springs or by the drainage-water of the deserts, held up by the impervious clay constituting the subsoil. In a few cases the moisture, he thinks, may be due to percolation from the Nile. The greatest altitude of the desert between Suez and Cairo is about 700 feet above the "ocean;" and its general character between the Red Sea and the Nile is that of a flattish irregular plateau rising towards the centre and terminating in each direction in abrupt escarpments. The flat marshy districts between Suez and Pelusium are stated, on the authority of Laborde, to be twenty-four feet below the sea-level.

The aspect of the valley and delta of the Nile varies with the seasons, presenting while the country is inundated a vast freshwater lake, studded with palm-shaded hamlets; and after the subsidence of the waters, exhibiting along the course of the river a line of brilliant verdure winding through higher sterile tracts. When the grain has been gathered, the prospect consists of one monotonous brown, dusty plain, traversed by the sluggish Nile. The dip of the country from the first cataract to the Mediterranean is, according to Mr. Wallace, only two inches in a mile; but the descent a little north of Assuan is seven inches, lessening however on approaching the delta, and the canal between the Nile and Alexandria, a distance of sixty miles, has not a single lock.

From the horizontal stratification of the rocks composing the greater part of Egypt, it is difficult, Mr. Newbold says, to trace any particular lines of elevation. The mural cliffs which flank the valley of the Nile to the vicinity of Cairo, there deviate towards the east and west, and similar but less abrupt cliffs flank both shores of the Red Sea. This horizontal formation is traversed by valleys and ravines or wadis, having a north and south, and east and west direction, or which intersect each other at right angles, the most considerable being that of the Nile.

In the eastern desert of Upper Egypt, Mr. Newbold traced these valleys to a north and south anticlinal line, caused by plutonic rocks which attain an altitude of 1000 feet above the sea-level; and their upheaval, he says, accounts for the intersecting systems of valleys, and illustrates forcibly the truth of Mr. Hopkins's observations on the laws of fracture. In the vicinity of the erupted rocks the sedimentary strata exhibit considerable proofs of disturbance, but as the distance

increases the inclination diminishes, proving, Mr. Newbold states, that the strata were elevated to their present position with no more force than was necessary to produce the fissures or valleys; and he adds, that in proportion as the horizontality is recovered, the frequency, depth, and extent of the fissures decrease.

Some of the valleys, as that of Kossier, are considered to have been widened by aqueous causes no longer in operation, and that of the Nile by the still continued erosion of the river; while others, as the Bahr-bila Maieh, or waterless river*, and that which separates the petrified wood formation from the Red Mountain, to have been formed entirely by them. The surface of these valleys is covered, for the greater part, with the detritus of the neighbouring rocks and of distantly transported rolled pebbles, which often rest on ledges and hills much above the general drainage-level. In the valley of Kossier, near the Red Sea, the gravel consists principally of pebbles of plutonic and hypogene rocks derived from the interior; but near the hills or to the westward of the parent rocks few of these pebbles are found, proving, the author says, the eastwardly direction of the transporting currents.

The natural drainage of the country is remarkably simple. The greater portion of the small quantity of rain which falls in Central and Upper Egypt is absorbed by the deserts and collected in natural basins like the Oases; the remainder is partly carried off by the great evaporation, and partly conducted to the Red Sea by transverse cracks on the eastern side of the anticlinal axis, or to the valley of the Nile by similar cracks on the western flank of that axis. The drainage of the Libyan desert is also effected through the valley of the Nile. The amount of water which escapes by these means is however so small, that the Nile throughout the last 1350 miles, or about one-half of its course, does not receive what may be termed a single tributary.

II. *Formations.*—The deposits of which Egypt consists, are arranged by Mr. Newbold under the heads of, 1. hypogene rocks with argillaceous schist, 2. breccia di verde, 3. lower sandstone, 4. marine limestone, 5. upper sandstone, 6. post-pliocene deposits, 7. drift, 8. volcanic rocks, 9. alluvial accumulations, 10. sand-drifts.

1. *Hypogene Rocks.*—These constitute a small portion of Egypt. Between the Red Sea and the Nile, Mr. Newbold observed them only in the latitude of Kossier (26° 8′) resting against granite in highly inclined or curved strata, and forming an east and west zone 30 miles in breadth. He is of opinion that the same beds may probably range south by east, hypogene rocks appearing at Gebel Zerbára (lat. 24° 30′). In a northerly direction they have been traced to the cataracts, resting on "granite."

Gneiss, with thin "veins" of marble, usually constitutes the lowest strata, which are overlaid conformably by micaceous, talcose, hornblende, and argillaceous schists and quartzite. Dykes or masses

* Mr. Newbold objects to the opinion entertained by some travellers that this valley was anciently the channel of the Nile, as it contains no rich, dark-coloured alluvium.

of basalt, greenstone, porphyry, and serpentine are associated with the whole series. All the hypogene rocks assume a crystalline character near the granite or trap, the gneiss and hornblende schist becoming garnetiferous and abounding in actynolite, both crystallized and compact; the talcose schist also passes into potstone and nephrite with iron pyrites, as at Mount Baram; the micaceous schists at Gebel Zerbára yield emeralds, avanturine, hæmatitic, and specular iron ore; and the clay-slate changes into basanite or flinty slate.

2. *Breccia di Verde.*—The argillaceous slate is overlaid conformably, in lat. 26° 8′, by the celebrated breccia di verde. This rock presents thick-bedded strata, which become more horizontal on receding from the granite, and is composed principally of angular and rounded fragments of greenstone, gneiss, porphyry, argillaceous and flinty slates, serpentine and marble, also sometimes of light green compact felspar and hypogene rocks, cemented by a slightly calcareous paste of various shades of green and purplish red. No fossils have yet been noticed in the rock. The cliffs composed of this breccia rarely exceed 200 feet in height above the level of the desert.

3. *Lower Sandstone.*—Above the breccia di verde occurs a sandstone of apparently limited extent, and confined to the southern part of Egypt, passing thence into Nubia. It is displayed on both flanks of the anticlinal axis between Kossier and Ghennah, reposing near Bir Anglaise conformably on greenstone; it is exposed also on the banks of the Nile, and, according to Lefevre, it ranges from a little south-west of Esneh (lat. about 26° 10′) nearly to Syene or Assuan, 70 miles, where it is dislocated by the syenite, and near its junction with that rock passes into a conglomerate and becomes agatiferous; it also, from the smallness of the fragments composing the breccia strata, and the altered crystalline structure of the mass near the plutonic rocks, often resembles certain porphyries, but the true nature of the rock is easily recognizable in the beds at a greater distance.

This sandstone varies from a loose siliceous aggregate with a felspathic, calcareous or ferruginous cement, to a compact quartz rock; and the pebbles in the interstratified breccia consist usually of chert, flinty slate, agate or jasper. Associated with the sandstone are occasionally thin beds of green and purple clay, containing gypsum and chloride of soda. Veins of white, brown, and amethystine quartz also traverse it, and copper as well as specular iron ore are stated to have been found in it near Hummamet. This sandstone was extensively used by the ancients. The vocal Memnon and many of the sphynges which line the dromos of the temple of Carnac consist of it.

Mr. Newbold hesitates to decide the geological position of the formation, though Ehrenberg considers it to be the representative of the Quader sandstein, and Lefevre of the Keuper or Marnes Irisées*.

4. *Marine Limestone.*—The sandstone is overlaid conformably by a marine limestone, which covers the greater part of Egypt, from near Esneh to below Cairo, or from lat. 25° 10′ to lat. 30° 2′, and from

* Bull. Soc. Géol. de France, tome x.

the Red Sea to the Libyan desert, with the exception of the tracts occupied by plutonic and hypogene rocks near Syene, and in the centre of the Egyptian desert, constituting for the greater part the basis of both deserts. Mr. Newbold considers the limestone on the eastern shore of the upper part of the Red Sea, extending to the base of Sinai and far into the Arabian desert, to be also of the same age. The dip is considerable as well as variable in the vicinity of the plutonic rocks; but there is scarcely any perceptible inclination in the beds composing the banks of the Nile; the general bearing of the dip is, however, decidedly towards the north.

The upper beds abound with Nummulites, and are generally hard and compact, but sometimes singularly honey-combed, apparently from the removal of the organic bodies. They are often siliceous, and considered, from effervescing slightly, to contain sometimes magnesia. The colour is buff or brown.

The lower beds have a cretaceous aspect, and contain, near Thebes and Bir Anglaise, nodular as well as tabular layers of chert, which are occasionally replaced by Egyptian jasper and agate, likewise innumerable small siliceous or cherty spheroids surrounded with a band, and called by the Arabs Nuktah, or drops. These concretions are sometimes united in pairs, and often present various modifications of a spheroid. Ehrenberg has not been able to detect any traces of organic structure in them, but he has noticed fragments of granite and other rocks. The lower beds yield also layers of earthy and crystallized gypsum, chloride of soda, arragonite, large deposits of stalagmite or Egyptian alabaster, near Tel el Amara (lat. 27° 43′) and in the Mokattem range, 8 hours from Benisuof; also in caverns fine stalactites, used in the arts. Among other mineral products, Mr. Newbold mentions sulphate of barytes, lead, crystallized sulphur, and nodules of carbonized vegetable matter. Interstratified with these lower beds are greenish and pale brown marls, the softer portions of which are used in washing, and the harder as whetstones. This limestone was employed in constructing the earliest Egyptian monuments.

According to Ehrenberg, the lower beds of this formation contain Infusoria and Foraminiferæ found in the Chalk of Europe; and to Lefevre*, Echinites at Esneh similar to those of Malta, also specimens of Hippurites, Placuna, and Vulsella, and a fish near Cairo; large Nautili, and numerous other testacea, with remains of crabs, fishes' teeth and corallines, were collected by Mr. Newbold. The author refers also to Mr. Bowerbank's observations that the Egyptian jaspers present no spongeous structure, but contain numerous Foraminiferæ resembling those found in chalk flints, yet difficult to distinguish from species obtained in the calcaire grossier†.

5. *Upper Sandstone.*—This formation occurs in horizontally stratified hummocks and patches resting on the marine limestone; and it has been traced from the Mediterranean far into the Nubian, Libyan and Bayúda deserts, and even into Abyssinia‡. The hummocks

* Bulletin Soc. Géol. de France, tome x. pp. 141, 234.
† See Phil. Mag. S. 3. vol. xix. p. 546.
‡ Lefevre, Bull. Soc. Géol. de France, tome x.

are considered to be the remains of once continuous strata. The sandstone varies from a red, white or yellow compact rock to a loose quartzoze grit, with a calcareous, felspathic or ferruginous cement, and associated with it is a conglomerate composed of quartz, chert and jasper, derived chiefly from the subjacent limestone; also beds of variously coloured marls containing gypsum and salt, and in which the natron beds of Egypt are situated.

Casts of marine shells were noticed by Mr. Newbold in the vicinity of Wadi Ansari, and trunks with smaller fragments of silicified trees occur in many parts of the Egyptian and Libyan deserts, particularly in the Suez desert, seven miles east by south from Cairo. This district, called the "petrified forest," is described in great detail. It consists of a sterile irregular plateau, which is considerably above the level of the Nile, lying on the slope of the Mokattem range, and it extends three and a half miles southwardly, and four miles eastwardly. Many of the trunks are scattered over the surface among rolled and angular fragments of dark grit, and pebbles of jasper, chert, quartz and sharp-edged fragments of silicified wood. The largest trunks occur in the greatest abundance on or near dark-coloured knolls, particularly towards the south-east portion of the area, where they lie like broken stems of a fallen forest, crossing each other at various angles; but the majority of the larger trees are directed towards the north-west. Two of the greatest, measured by the author, were 48 and 61 feet in length, and $2\frac{1}{2}$ and 3 feet in diameter; but the lesser fragments are generally from 1 to 3 feet long, and 4 to 12 inches in diameter. Among the fractured trunks which lay broken transversely on the sand-hills, Mr. Newbold noticed many with the edges sharp, and in nice adaptation, though the fragments were several feet apart.

A few specimens are imbedded horizontally in the sand and associated conglomerate, and a still fewer occur in a vertical position rising from 12 to 20 inches above the surface. Mr. Newbold cleared the sand from one of these stumps, and ascertained that its lower part was imbedded in the subjacent conglomerate; but it exhibited no traces of roots.

The trunks, which are rarely flattened and never invested with coaly matter, are branchless, and in general knotless; though in some specimens Mr. Newbold traced places for the insertion of branches; roots also are wanting, but among the masses enclosed in the sand some were found, which bore strong resemblance to the bulbous base of palms, and others which assimilated to the tortuous roots of exogenous trees. Internally the trunks exhibit a concentric structure, though externally they resemble the present palms of Egypt. Some specimens examined by Mr. R. Brown were decided to be dicotyledonous, and not coniferous; but one brought from the Nubian desert by the Rev. Vere Monro is stated to exhibit that structure. Indications of a jointed appearance are mentioned, but Mr. Newbold is of opinion that this calamite or reed-like structure may be due to contraction during the process of silicification. Instances of decay at the time the trunks were imbedded the author also noticed, the

interior being partly filled with grit and conglomerate; and he mentions cases in which all ligneous structure had disappeared. The silicified wood varies in character from a white opake crust, which crumbles when handled, to agate and flint, and in colour from white to grey, brown and red. No decided seed-vessels or traces of leaves have been found.

The author then describes the structure of Gebel Ahmar, situated on the northern limit of the "Fossil Forest," and of the intervening valley. Gebel Ahmar is an irregular ridge, a mile in length and half a mile in breadth, rising to the height of about 150 feet above the general level of the desert, and it is composed of conglomerate, grit and sandstone, the prevailing colour of the strata being red (Gebel Ahmar, Red Mountain).

The stone has been so extensively quarried, and the mounds of rubbish are so numerous, that the original outline of the ridge has been obliterated; and its present rugged, conical aspect is due to those causes, and not to a supposed volcanic origin. The sandstone reposes, as elsewhere, on the marine limestone, passing near the line of junction into an ochreous, reddish and yellow clay, which contains veins of fibrous gypsum, selenite, salt, and, it is said, barytes.

Both the sandstone and limestone abound in caverns, "the resort of the hyænas that nightly prowl among the burial-grounds without the walls of Cairo. One of these dens, into which" Mr. Newbold descended, "contained the recent dung of the animal intermingled with human and other bones."

The valley which intervenes between Gebel Ahmar and the "Fossil Forest" is excavated in the sandstone, the subjacent limestone being in some places exposed.

The following inferences are drawn by Mr. Newbold from the phænomena presented by the deposit of petrified trees:—

(1.) He is of opinion that this part of Egypt has twice formed the bed of the ocean, and been twice elevated above the surface of the water.

(2.) That the fossil trees lived between these epochs, when they were submerged or drifted into the ocean, and were covered up by a bed of rolled pebbles or sand; and that they were afterwards raised to their present position.

(3.) That the elevation of the strata was effected gently and gradually, as the horizontal position is maintained.

(4.) The retiring water is supposed to have removed the looser portions of the once continuous strata, and to have dispersed them with fragments of the fossil trees over the surface of the Egyptian and Libyan deserts, constituting the present accumulations of gravel and saline sands.

(5.) From the little-worn aspect of the trunks, as well as the angularity and "nice adaptation" of many of the fractured portions near Cairo, it is inferred, that, in that locality at least, the specimens rest at no great distance from the spot on which they were silicified; and from the vertical position of a few of the trunks, that they pro-

bably occur where they grew; but until the vertical stems are traced down to roots fixed in a given stratum or at certain levels, marking, as in the Portland "dirt-bed," the ancient surface of dry land, Mr. Newbold hesitates to admit the hypothesis that the Cairo fossil deposit is the site of a submerged forest.

Reposing horizontally, and at the height of 300 feet, on the inclined limestone of the Gebel Ataka range which skirts the shore of the Red Sea below Suez, is a calcareous conglomerate, which Mr. Newbold thinks may represent the sandstone formation, as it rests on the marine limestone, and contains similar pebbles; but it contains no silicified wood, nor any other fossils, except such as have been derived from the subjacent limestone.

6. *Post-Pliocene Deposits.*—Around the head of the Gulf of Suez, as well as between the Red Sea and the cliffs which skirt its western shore, is an interrupted fringe rising in some parts to a height of 60 feet, with an extreme breadth of four or five miles, composed of calcareous deposits containing the remains of testacea, radiaria and corals, which now inhabit the Red Sea. Kossier and several other towns stand upon this formation. It is suspected by some observers, on account of the obliteration or shallowing of anciently deep harbours, that the process by which the fringe was raised above the level of the sea is still in operation, and Mr. Newbold is of opinion that the forces which effected the upheaval acted gently and gradually. He objects, however, to the inference that the isthmus of Suez has been recently raised, on account of the difference in the faunæ of the Mediterranean and the Red Sea.

Among the post-pliocene formations, the author includes the accumulations now forming around the Red Sea and in the Mediterranean on the shores of Sicily, Greece, Asia Minor, &c. On the west shores of the Red Sea he has noticed them five or six feet above high-water mark, overlying a raised coral beach. They sometimes enclose bones of the camel; and in the island of Rhodes Mr. Newbold observed in a similar formation fragments of ancient pottery.

In the valley of the Nile, on the plain of Benihassan, myriads of Nummulites, washed from the overhanging limestone, are partially re-cemented by calcareous matter deposited from springs and form layers which alternate horizontally with others composed of clay, sand and gravel, the whole in some places attaining a thickness of more than 30 feet. In the valley of Kossier, beds of gravel and other detritus are gradually becoming consolidated by a calcareous or ferruginous cement derived from percolating water; and in the cliffs skirting the Mediterranean, between Alexandria and Aboukir, Mr. Newbold observed a bed of bleached bones, derived from Roman and Greek cemeteries, with an intermixture of more modern human remains, overlaid by a layer of occasionally agglutinated sand or gravel, sometimes from three to four feet thick.

7. *Drift.*—Under this head the author includes, 1st, the saline sands and gravel of the deserts, derived in great part, he believes, from the fossil-wood sandstone formation, but generally much influenced in each portion of the deserts by the character of the rocks in the im-

mediate vicinity; and 2ndly, the gravel beds which cover the raised coral beach of Kossier and the limestone cliffs of the Red Sea near the Jaffatine group, also the detritus resting on the elevated platform of the Libyan desert near Dendera, the materials composing the whole of which consist of far-transported plutonic and metamorphic pebbles, intermingled with others derived from adjacent formations.

8. *Volcanic Rocks.*—After alluding to the supposed volcanic cones or extinct craters in the desert between Cairo and Suez, and to others said to exist in the vicinity of Dakkeh, situated in the Nubian desert 69 miles from Syene, Mr. Newbold proceeds to describe the trap and porphyry dykes which in Upper Egypt penetrate all the rocks from the lower sandstone to the granite, and have been already noticed in the account of the formations through which they pass; the author, however, observes in addition, that the relative age of the trap is defined by the upper or fossil-wood sandstone being undisturbed, and by its sometimes containing pebbles of the trap.

Granitic or syenitic rocks are of rare occurrence in Egypt, appearing only at the cataracts of Syene, and in the desert between the Nile and the Red Sea, forming the anticlinal axis (lat. about 26° N.); and according to M. Trivin, still further north in the same desert, in about the latitude of Benisuof (29° 10′ N.). This locality, Mr. Newbold thinks, may be that mentioned by Savary. Sir G. Wilkinson has likewise traced them to lat. 28° 26′, where they form the peak of Gebel Tenaset; and the same author states that the extreme height attained by these rocks in Gebel Gharib (lat. 28° 10′) is 5000 feet above the sea.

Respecting the relative period of their elevation, Mr. Newbold is of opinion that it was subsequent to the deposition of the inferior sandstone and limestone which occur on their flanks in inclined strata, and prior to that of the superior horizontal sandstone. He is likewise of opinion that the plutonic rocks were upheaved through once continuous solid strata of sandstone and limestone, on account of the absence of granitic veins in those deposits and the occurrence of breccias along the junction line of the igneous and sedimentary formations. He carefully examined the limestone and sandstone for imbedded pebbles derived from the granite or syenite, but without success. Granitic veins penetrate the gneiss.

9. *Alluvial Accumulations.*—These deposits Mr. Newbold describes under, 1st, the mud of the Nile, and 2ndly the Delta; but he alludes also to the vegetable soil of the Oases, to the detrital soil washed down from the rocks, and to the greyish soil accumulated generally around the ruins of ancient cities, due partly to the decay of animal and vegetable matter, partly to the mouldering ruins; likewise to the ammoniacal and nitrous salts formed in the deserts where caravans have halted, and which have been collected from the earliest times.

(1.) *Mud of the Nile.*—This accumulation varies with the nature of the formations over which the Nile flows, and is therefore, Mr. Newbold observes, not merely the result of the spoils of Abyssinia. To this cause he also ascribes the discrepancies in the analyses of the mud. Above Thebes, below the granitic and sandstone formations

of Nubia, and on the southern limit of Egypt, it contains more silex and less calcareous or argillaceous matter than at Cairo, which stands on the great limestone deposit, and in the Delta, which rests on that formation. It varies also in texture and composition, according to its position relative to the main channel of the river and the force of the current. The finest mud, as that of Ghennah, is generally dark brown passing to lighter shades; it is also highly tenacious, retentive of moisture, effervesces, and fuses *per se*, with extrication into a greenish glass. The annual deposit or layer varies in thickness in the same situation from an inch to a few lines, the upper part being generally lighter than the lower; and each layer is separable from that above or beneath it; but the deposition of one year is frequently removed by the flood of the next.

Mr. Newbold does not know if the thickness of the mud in the centre of the river's bed has been ascertained; the greatest accumulation in a transverse section being near the stream's channel; but in Upper Egypt he has measured cliffs composed of it forty feet in height; and the average thickness in Middle Egypt is thirty feet, while at the apex of the Delta it is eighteen feet. According to Sir G. Wilkinson, the deposit has increased during the last 1700 years at Elephantine in Upper Egypt nine feet, at Thebes seven feet, and at Heliopolis five feet ten inches; but the amount of accumulation diminishes in general more rapidly towards the Delta and Mediterranean. All calculations, however, on the progressive rate of increase throughout Egypt, deduced from the actual addition around the bases of nilometers, statues or buildings, in particular localities, are liable, Mr. Newbold says, to uncertainty, on account of the shifting of the river's bed, and the intermingling of the drift sand of the desert. Moreover, the alluvium at the foot of these monuments has been disturbed by the plough and spade of cultivators; and in most cases it has not been proved at what period the Nile reached these bases; but judging from the thickness of the annual layers, of which the author has counted upwards of 900 in the cliffs of the Nile, he concludes that the yearly deposition has not varied in the aggregate for the last thousand years. It is equally difficult, he adds, to calculate the progressive superficial extension of the mud.

Few pebbles or detritus of any size are found in Lower Egypt and in the Delta, and only the finest ingredients escape into the Mediterranean, but Mr. Newbold has observed the sea coloured by this drifted matter to the distance of forty miles from the shore. The northern or Etesian winds, which commence about May, or the period of the inundation, retard, he says, the downward freshes, and contribute materially to the accumulation of the mud upon the land, as well as to the silting up of the embouchures of the river, by raising the level of the Mediterranean along the coast, and checking the currents in the estuaries. Near the mouths of the Nile the mud is intermingled with marine sand, and contains Mediterranean species of Mollusca, associated with terrestrial and fluviatile remains. According to Ehrenberg, the river-mud contains an immense number of infusoria.

The action of the Nile on its eastern bank, arising from a difference in the level at the base of the Arabian cliffs and the prevalence of westwardly winds, is shown to be considerable. Many monuments of Koum Ombos have been carried away, and the remainder are threatened; further down, the ancient quay, and the temple at Luxor, are in great danger; and the ruins of Gou-el-Kebir have been in part destroyed by the encroachments of the river, the traditional channel of the Nile being nearly a mile to the westward. Other changes are also mentioned.

(2.) *Delta of the Nile.*—On account of the absence of all marine remains from the mud covering the middle and upper portions of the Delta, Mr. Newbold infers that the present alluvium must have been deposited, for the most part, on a surface previously above the level of the Mediterranean; and he is also of opinion that other causes than the deposition of mud have tended to the formation of the Delta. The coast-line, he shows, consists chiefly of banks of marine sand, and a recent marine limestone: ancient Alexandria also stood on the calcareous rock of the Libyan desert, but the modern city is built on the recent marine sands and calcareous strata, occupying the position of the great harbour. Foah, which at the commencement of the fifteenth century was situated at the Canobic mouth of the Nile, though now a mile from it, and the present inland position of Rosetta, Micopolis and Taposiris, Mr. Newbold says, must likewise be ascribed, in great measure, to the intervention of marine sand-banks.

The increase of soil from the waters of the Nile is much slower in the Delta than in the valley of the river, being spread over a much greater extent; and though a considerable quantity of the suspended matter is carried into the Mediterranean, yet the author does not think that the submarine accumulation of the Delta can be very rapid.

10. *Sand-drifts.*—At a short distance from both the Red Sea and the Mediterranean, the shores are occasionally studded with dunes or hills derived chiefly from the drifting of sand-banks thrown up by the waves. In considering the nature of the sands of the deserts, and their encroachments, the author dwells upon the effects of the strong north-westerly and westerly winds, which blow during nine months of the year; and on the agency of the little whirlwinds which prevail chiefly in the hot season, and transport not merely the finer particles of sand, but seeds of plants, and marine, fluviatile and land shells. With respect to the effects of the sand-flood, the author alludes to the more considerable encroachments and to their increasing influence, likewise to the natural impediments to their progress presented by the rugged ravines and cliffs of the western desert, and by the Nile: and lastly, he states that the accounts of whole caravans having been overwhelmed by clouds of drifting desert-sands are greatly exaggerated; the effects having been confined to infirm or over-fatigued travellers and animals who were unable to keep pace with the caravan.

XXXVIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 73.]

April. 6, 1842. A PAPER was first read on the genus Tetracaulodon by Mr. Koch, communicated by the President.

Mr. Koch commences by stating, that a difference of opinion having existed in the scientific world respecting the genus Tetracaulodon of Dr. Godman, and as only a few weeks previously, in a memoir read before the Society, the Tetracaulodon was pronounced to be simply the male of the Mastodon*, he conceives it to be his duty to make public the results of his researches, and which fully prove, in his opinion, that the Tetracaulodon is a distinct genus, consisting of several varieties.

The author declares that he has examined with the greatest accuracy all the inferior jaws of the Mastodon preserved in the collections of the United States, but has never seen any specimens with the least traces of a tusk; and he adds, that Dr. Hays of Philadelphia, after a careful inspection of at least forty jaws, had arrived at the same conclusion. According, therefore, to the common laws of nature, it is highly improbable, observes the author, that the Mastodon was such an exception that not one male existed among forty females.

The Mastodon of Philadelphia, Mr. Koch says, is a male, according to the construction and size of the pelvis and the magnitude of the tusks in the upper jaw, yet there are no traces of tusks in the lower jaw; and the specimen at Baltimore, which is considered to be indisputably a male, is also destitute of inferior tusks. The author likewise states that he has uniformly found the jaws of young Mastodons to be very rare; that those which he has seen have no indications of tusks; and that he has in his possession the lower jaw of a young Mastodon, mentioned by Dr. Hays, which has no tusks. Hence he infers that there are young Mastodons, as well as Tetracaulodons.

Mr. Koch then proceeds to draw attention to "some important points" which he believes have not been noticed.

Admitting, for the sake of argument, that the male Mastodon was the possessor of the small tusks only eight or twelve inches long in the lower jaw, he says it would have been utterly impossible for that animal, with his enormous upper tusks and short neck, to have reached the ground with them; yet these small lower tusks, he states, show that they were much used in rooting and grubbing, and therefore must have belonged to an animal which had equally short upper tusks. To substantiate this inference he calls attention to three species of Tetracaulodon, the first discovered by Dr. Godman, the two others by himself.

1. *Tetracaulodon Godmanii.*—This species having been described in detail by Dr. Godman, Mr. Koch only points out the great difference of the maxillary and nasal bones, as well as the additional foramina near the malar bone, which are wanting in the Elephant

* See p. 56.

and Mastodon. He says that in his collection there is a fragment of a lower jaw of this species with a tusk which shows very distinctly the difference of the lower tusks in the *T. Godmanii* from those of the *T. Kochii,* the character consisting in the root of the tusk being pointed; and he states that he has not been able to discover the place occupied by the dental nerve.

2. *Tetracaulodon Kochii.*—Of this "species" the author possesses three lower jaws of adults, and one of an extremely young animal; also two upper tusks belonging to two different individuals, and two which belonged to one Tetracaulodon. Mr. Koch states that he found the roof of a mouth of this species perfect, with its six molar teeth and the tusks in their "maxillary bones" resting on the lower jaw which retained a tusk in the alveolus, but that the veins of iron intersecting the deposit prevented him from extracting this valuable specimen entire, but that he secured the upper tusks and grinders, and the lower jaw with the tusk in its alveolus. It does not require a close examination, the author says, to perceive that the animal to which these remains belonged was neither male, female, nor young Mastodon, or Missourium, the whole inner and outer conformation of the upper tusks showing that they were calculated to be used in harmony with the lower tusk in grubbing and rooting. Hence the author infers that the Tetracaulodon lived principally on roots, whereas the Mastodon, he says, consumed the large upper herbage. The superior tusks of this specimen measure only 19 inches in length, one-third having been "concealed in the skull," and their greatest circumference is $9\frac{1}{2}$ inches. They possess the peculiarity of being larger at the apex than the base, the former also exhibiting indisputable marks of having been much used during the life of the animal. They were slightly curved upwards. The enamel on the root is very thin, but it increases rapidly towards the extremity, where it is extremely thick. "The bulb for the dental nerve" is stated to be small and to terminate suddenly in a point.

With reference to the tusks of the lower jaw, Mr. Koch agrees to the view that the young animal possessed two, and the adult only one, situated on the right side. It was, he says, slightly curved downwards and then upwards, and in both old and young animals possessed the peculiarity of being equal in circumference at both extremities. The bulb for the nerve which nourished the tusk resembles minutely that of the upper tusks, both in adult and immature animals; and Mr. Koch is of opinion that this peculiarity of the lower tusks gives rise to "a suspicion of not merely a different variety of the Tetracaulodon, but even of a new genus."

3. *Tetracaulodon Tapiroides.*—This specific distinction Mr. Koch has founded on the first grinder resembling that of the Tapir. He possesses the greater part of a skull and its two tusks, which were in their proper position when he found the specimen. The tusks are described as perfectly straight, but bent downwards like those of the Morse, to which, the author says, they bear a strong resemblance; and, from their worn condition, he believes that the animal

lived on the roots of water-plants, &c. They are covered with a thick coat of enamel, and were concealed for one third of their length in the sockets; they are also stated to be large in proportion to the size of the skull; and the bulb is said to resemble that of the foregoing variety.

Mr. Koch then calls attention to some vertebræ in his collection belonging to a gigantic animal, but to neither the Mastodon nor the Elephant. They consist, he says, of an extremely well-preserved lumbar, and a second cervical vertebra. The most striking character of these bones is stated to be the great size of the foramen in reference to the smallness of the body, the former being double the dimensions of that of the Mastodon. The author also says that the cervical vertebra presents two peculiar "cavities, situated on the right and left of the root of the toothlike process," and which he "considers to have been for the reception of two unusual muscles, to enable the animal to perform a peculiar motion with the head." As he found these vertebræ in the same deposit from which he obtained the skull and jaw, and as he conceives that the Tetracaulodon must have possessed the power of moving head and neck in a peculiar manner whilst grubbing for its food, Mr. Koch believes that these vertebræ belonged to that animal.

ROYAL ASTRONOMICAL SOCIETY.

Dec. 9, 1842.—The following communications were read:—

I. A Letter from Professor Henderson to the Secretary, on the Parallaxes of certain Southern Stars.—This will be found in the Monthly Notices of the Society for December 1842, vol. v. p. 223.

II. Observations of the Beginning and End of the Solar Eclipse of July 7, 1842, communicated by C. Runker, Esq. in a letter to Dr. Lee.

III. Occultations observed at Yarmouth, by Arthur Utting, Esq., communicated in a letter to E. Riddle, Esq.

Abstracts of the two preceding communications will also be found in the Society's Monthly Notices for December 1842.

IV. Sequel to a paper "On a new Method for greatly facilitating the Computation of the Moon's Co-ordinates." By S. M. Drach, Esq.

The object of this paper is to present, in a practical shape, the transformation of the lunar equations which had been suggested by the author in his former paper for facilitating the computation of the moon's co-ordinates. Though the facilitation did not reach the extent at first anticipated, still it is hoped by the author that much labour will be saved to the computer of the places of the moon by the use of the method proposed.

The paper is accompanied by two skeleton forms, representing the details of the computations necessary for computing the co-ordinates by the use of the tables proposed by the author.

Jan. 13, 1843.—The following communications were read:—

I. Translation of a Letter from Professor Hansen to G. B. Airy, Esq., the Astronomer Royal, on a New Method of Computing the Perturbations of Planets, whose Eccentricities and Inclinations are not small. Communicated by G. B. Airy, Esq., A.R.

"Sir,—I hasten to communicate to you a piece of astronomical intelligence of some importance. You are aware that all the methods that we possess for calculating the perturbations of the planets suppose that the eccentricities and inclinations are small, and that for those of the celestial bodies, which move in orbits very eccentric and very much inclined, we have been hitherto obliged to calculate the differentials of the perturbations for a great number of points of the orbits, and to integrate them by mechanical quadratures. I have just now discovered a method by which we can calculate the absolute perturbations,—that is to say, the perturbations for any time whatever, whatever be the eccentricity of the ellipse and the inclination of the orbit. For a first example of this method, I have calculated the perturbations of the comet of Encke produced by Saturn. The series to which my method leads are of such rapid convergence, that the perturbations of the longitude contain only forty-six terms, and the perturbations of the radius vector and of the latitude, somewhat fewer than this. I have reason to believe that it is impossible to reduce them to a less number of terms. Instead of writing here all the terms explicitly, allow me to represent generally the value for the time of perihelion passage.

"Here then is the first result of this kind, in which $n\,\delta t$ represents the perturbations of the mean longitude; u those of the hyperbolic logarithm of the radius vector, expressed in seconds, of the above-mentioned comet; g' the mean anomaly of Saturn; and t the time, of which the unit is a Julian year.

$$\begin{aligned} n\,\delta t = -0''\!\cdot\!06 - 1''\!\cdot\!7152\,t &+ 1''\!\cdot\!56 \sin g' - 14''\!\cdot\!23 \cos g' \\ &+ 23\cdot41 \sin 2g' + 20\cdot65 \cos 2g' \\ &- 6\cdot39 \sin 3g' + 8\cdot52 \cos 3g' \\ &- 2\cdot65 \sin 4g' - 2\cdot89 \cos 4g' \\ &+ 1\cdot43 \sin 5g' - 0\cdot96 \cos 5g' \\ &+ 0\cdot32 \sin 6g' + 0\cdot55 \cos 6g' \end{aligned}$$

$$\begin{aligned} u = -1''\!\cdot\!05 - 0''\!\cdot\!1611\,t &+ 0''\!\cdot\!61 \cos g' + 8''\!\cdot\!86 \sin g' \\ &+ 33\cdot10 \cos 2g' - 29\cdot85 \sin 2g' \\ &- 9\cdot01 \cos 3g' - 11\cdot47 \sin 3g' \\ &- 3\cdot41 \cos 4g' + 3\cdot90 \sin 4g' \\ &+ 1\cdot74 \cos 5g' + 1\cdot11 \sin 5g' \\ &+ 0\cdot32 \cos 6g' - 0\cdot82 \sin 6g' \end{aligned}$$

"In the *Astronomische Nachrichten*, vol. ix. No. 211, M. Encke has published for three periods the separate perturbations of this comet for each planet, and for the time of perihelion passage. We are therefore able to compare these perturbations with their preceding general value. But in this comparison it is necessary to remark, that in the calculation of the perturbations by mechanical quadratures, there arises in the perturbations of the epoch of the mean

anomaly a term proportional to the time, which does not exist in the absolute perturbations, and that it is, consequently, necessary to determine the value of and to subtract this term. This being premised, x being the value of this term for a whole revolution of the comet; $\varkappa$ the number of revolutions; $\Delta\, m$ the perturbations of the epoch of the mean anomaly; $\Delta\, \pi$ those of the longitude of the perihelion; $\Delta\, \Omega$ those of the longitude of the ascending node; $\Delta\, \phi$ those of the angle of the eccentricity ($e = \sin\phi$); $\Delta\, \mu$ those of the mean motion; i the inclination; we have

$$n\,\delta\, t = \Delta\, m + \frac{(1-e)^2}{\sqrt{1-e^2}}\,\Delta\,\pi - 2\sin^2\frac{1}{2}\,i\,.\,\frac{(1-e)^2}{\sqrt{1-e^2}}\,\Delta\,\Omega - \varkappa\, x$$

$$u = -\frac{2}{3}\,\frac{\Delta\,\mu}{\mu} - \Delta\,\phi\,\sqrt{\frac{1+e}{1-e}}.$$

"By substituting in these expressions the numerical values, which M. Encke has given at the place above quoted, we find for the period

Of 1819, Jan. 27·25 to	1822, May 24·0	$n\,\delta\, t = -\ 67''{\cdot}81 - x$;	$u = +\ 94''{\cdot}11$
...	1825, Sept. 16·3	$= -\ 79{\cdot}30 - 2\,x$	$= +100{\cdot}78$
...	1829, Jan. 9·72	$= -124{\cdot}42 - 3\,x$	$= +\ 54{\cdot}54$

"For these four times we have the mean anomaly of Saturn, augmented for the sake of greater correctness by the great inequality; thus

$$\begin{aligned} g' &= 266^\circ\ \ 8' \\ &= 306\ \ 42 \\ &= 347\ \ 13 \\ &= \ \ 27\ \ 44 \end{aligned}$$

"If we substitute these values, as well as the values of t, 0; 3·322; 6·636; 9·952, in the preceding expressions of the absolute perturbations, we find for these four times,

$$\begin{aligned} n\,\delta\, t &= -25''{\cdot}97 \qquad & u &= -66''{\cdot}38 \\ &= -46{\cdot}59 & &= +28{\cdot}94 \\ &= -\ \ 8{\cdot}12 & &= +34{\cdot}81 \\ &= -\ \ 3{\cdot}57 & &= -12{\cdot}52 \end{aligned}$$

"If we subtract from these the first-mentioned values, we obtain the perturbations for the three above-mentioned revolutions of the comet. Thus

$$\begin{aligned} n\,\delta\, t &= -20''{\cdot}62 \qquad & u &= +\ \ 95''{\cdot}32 \\ &= +17{\cdot}85 & &= +101{\cdot}19 \\ &= +22{\cdot}40 & &= +\ \ 53{\cdot}86 \end{aligned}$$

"By comparing these values of u with those given before as found by M. Encke, we obtain the following differences:—

$$\begin{aligned} &+1''{\cdot}21 \\ &+0{\cdot}41 \\ &-0{\cdot}68 \end{aligned}$$

"By comparing in the same way the values of $n\,\delta\, t$, we find immediately

$$0 = +20''{\cdot}62 - 67''{\cdot}81 - x \quad \text{or} \quad 0 = -47''{\cdot}19 - x$$
$$0 = -17{\cdot}85 - 79{\cdot}30 - 2x \qquad 0 = -97{\cdot}15 - 2x$$
$$0 = -22{\cdot}40 - 124{\cdot}42 - 3x \qquad 0 = -146{\cdot}82 - 3x$$

"Hence we get $x = -48''{\cdot}711$, by the substitution of which there result the following differences:—

$$+1''{\cdot}52$$
$$+0{\cdot}27$$
$$-0{\cdot}69$$

of the perturbations of longitude. These differences, as well as those of the perturbations of the radius vector, are smaller than might have been expected, when we reflect on the total diversity of the methods employed, and the long calculations which the method of mechanical quadratures requires. Besides, my method is so simple that I am astonished at not having discovered it long ago; I have employed only eight days for the calculation of the preceding perturbations, the general expression of which belongs to every point of the orbit of the comet. I have thus succeeded in solving this problem, of which we till the present time possessed no solution.

"I beg you to communicate this letter to the Royal Astronomical Society, and to the Royal Society of Sciences, and to accept the expressions of high consideration, with which I am, Sir,

"Your very obedient servant,

"Gotha, Dec. 14, 1842." "P. A. Hansen."

II. On a new Arrangement of a Vertical Collimator attached to the Altitude and Azimuth Instrument. By W. Simms, Esq. An abstract of this paper will be found in the Monthly Notices for January 1843, vol. v. p. 230.

III. Description of a Universal Instrument made by M. Ertel of Munich, and presented to the Society by Alexis Greig, Esq., Vice-Admiral in the Imperial Russian Navy. By M. Ertel. Translated from the German by Mr. Charles Knorre, and communicated by Admiral Greig.

This paper commences with a detailed statement of the cautions to be used in taking the instrument out of its cases, and of fitting it up for observation; and gives minute directions for rectifying and using it. It is accompanied by two drawings, the first of which is that of a projection parallel to the plane of the horizontal circle of the instrument; and the second exhibits in detail some of the essential parts of it.

IV. Occultations observed chiefly at Ashurst in the Year 1842. By R. Snow, Esq.

V. Observations on the (apparently periodical) Variations in the Lustre of certain Stars of the First Magnitude. By T. Forster, Esq.

Abstracts of the two preceding communications also are given in the Monthly Notices for January 1843.

LONDON ELECTRICAL SOCIETY.

[Continued from vol. xxi. p. 485.]

Jan. 17*.—"On Assaying by Galvanism," by Martyn J. Roberts, Esq., F.R.S.E., M.E.S. This consists in employing a simple galvanic pair, the positive element of which is the metal next in affinity for oxygen to that to be extracted; as a pair of silver and copper to be employed in extracting silver from a solution containing silver, copper, and iron: this method was perfected and practised many years ago.

"Dissection of a second *Gymnotus Electricus*; and the Anatomy of the Torpedo," by H. Letheby, Esq., B.M., A.L.S., Curator to the Museum of the London Hospital. The author follows out the views developed in his former paper, and touches on those points which were not accessible to him from the condition of the former specimen. The Society are indebted for this, as well as for several other specimens, to the liberality of Walter Hawkins, Esq., who has expressed his determination to persevere until he succeeds in presenting to the Society a *living* specimen of the Gymnotus.

"New Voltaic Battery," by Schœnbein. This consists of zinc and passive iron, or of active and passive iron, in either case excited after the manner of a Grove's battery. The power of such arrangement is said to be very great. Its economy is a matter of importance; and the value of the salt produced (sulph. ferri) is not to be overlooked.

"Report of Mr. Armstrong's Electrical Steam Apparatus," by L. L. Boscawen Ibbetson, Esq., K.R.E., M.E.S., &c. This instrument had power, under the unfavourable circumstances of a wet day, to produce a 15-inch spark, and to give 120 spontaneous discharges per minute to a Leyden jar 5 inches diameter, and coated to the height of $6\frac{3}{4}$ inches.

"Disturbance of Electric Equilibrium," by Martyn J. Roberts, Esq.

Mr. Weekes's Register for December.

LITERARY AND PHILOSOPHICAL SOCIETY OF LIVERPOOL.

Nov. 14, 1842.—Dr. Sutherland read a paper "On the Origin and Progression of Glaciers," in which, after giving a general view of their natural history, he proceeded to the examination in detail of the new facts in regard to their structure and motion which have recently been discovered by Professor Forbes and M. Agassiz. The facts bearing most closely on the theories of progression were divided as follows:—

A. Facts in regard to Structure.—1st. It has been shown by Agassiz that glacier ice is highly porous, and admits of the free circulation of water through its structure. 2nd. That the quantity of water in the ice is greater during the day than during the night. 3rd. That the surface of the ice is constantly melting whenever the atmospheric temperature is above 32° Fahr., and that the quantity of water passing through the glacier is found to increase when the atmospheric temperature rises and to diminish when it sinks. 4th. That the temperature of the ice of the glacier is at all ascertained depths 32° Fahr., at least during the season of progression, and that it varies very little if at all from that temperature. 5th. That

* The Proceedings for December 1842 will be given in our next.

we owe to Professor Forbes the description of a very beautiful structure in glaciers to which he has applied the term Conoidal, one aspect of which is the appearance of loops on the surface of the glacier, the apices of which point downwards in the direction of the glacier's motion, and which Professor Forbes states "give the idea of fluid motion, freest in the middle, obstructed by friction toward the sides and bottom."

B. Facts in regard to Motion.—1st. That glaciers do not progress during winter. 2nd. That they progress during the warmer months at observed rates of 200 feet and upwards a year, according to circumstances of temperature, inclination of bed, &c. 3rd. That glaciers progress more rapidly in the centre than at the edges, and more rapidly at the lower than at the upper part of their bed. 4th. That glaciers progress more rapidly during the day than during the night, and that their motion is in proportion to the quantity of water supplied to their porous and capillary structure by the melting of their surface by atmospheric temperature and rains, so that their motion increases or diminishes according as the rise or fall of the atmospheric temperature increases or diminishes their supply of water. 5th. That the surfaces of glaciers are removed by melting and evaporation to the extent of several feet a year without any alteration in their level. 6th. That the peculiar conoidal structure already mentioned, as well as other facts observed by Professor Forbes, appear to establish the opinion entertained by him, that glaciers progress after the manner of semifluids.

The author next proceeded to consider the theories which have been advanced to explain glacier-motion. These are two in number,—the Dilatation Theory and the Gravitation Theory. The first of these assumes that the water infiltrated into the glacier loses its caloric of fluidity, and in freezing expands longitudinally so as to force the glacier to advance, and vertically so as to maintain the thickness of the ice. Many objections were urged against this theory, two of the principal of which were the following:—1st. That the temperature of the glacier had been proved by Agassiz to be 32° Fahr. during its period of advance, and that it had never been proved that water proceeding from melted ice would freeze when brought in contact with ice at 32° Fahr. And it had besides been proved that the freezing point of water falls below 32° Fahr. under diminished atmospheric pressure, which we know to exist at the altitude of glacier regions. The occurrence of liquid water also at all depths in the glacier was shown to militate directly against the theory. 2nd. That in order to account for the ice always retaining its level, although the surface is constantly removed by melting and evaporation, the dilatation theory requires that *all* the water arising from this melting be refrozen in the substance of the glacier, whereas the greater part of it escapes by the crevices to the bed of the glacier, to form the glacier torrent, and only a small part enters the substance of the ice, and thus in one essential particular the theory is wholly inapplicable.

The gravitation theory as promulgated by Saussure was shown

to be inapplicable, on account of its resting on important errors in matters of fact, and wanting other facts which had been ascertained since Saussure's researches were published.

The author next proceeded to explain the following theory, which he thought capable of including all the phænomena as yet known. During winter glaciers repose on their inclined beds in a condition of perfect rest. When warm weather arrives the atmospheric temperature rises above 32° Fahr. The joint action of the air, rains and the solar rays, melts the ice and snow at the surface of the glacier. Part of the water finds its way by the crevices to the glacier bed, and there acting on the ice fits it for motion. Another part at the same moment percolates the spongy ice, and incorporating with its structure impresses the property of mobility on its parts and molecules. The whole mass of the glacier is now in a condition to obey the ordinary laws of fluids. It descends the mountain side like any other semi-fluid, by the action of gravity, and perhaps the head pressure of the Nevé. When the supply of water is diminished by cold its progress is also diminished. When the supply is increased by heat its progress is also increased, and that as a necessary consequence of the function which the water performs. The glacier is consumed by the melting of its surface and lower extremity, and its level preserved by molecular movements within it, just as in the case of other fluids in channels.

When the summer and autumn are gone, the temperature of the air falls to the freezing point or below it, the melting of the surface ceases, the supply of water to the spongy structure of the glacier is cut off; it loses its property of molecular motion, it ceases to be a semi-fluid, it becomes rigid, and its weight and friction afford effectual resistance both to the action of gravity and to the head pressure of the Nevé from the winter snows; it returns to its state of perfect rest, and there it remains till next summer's heat melts its surface and affords water, which is its pabulum of motion. The hydrostatic pressure of the water in the ice is an element which very probably acts an important part in these phænomena, both by exerting its force and by its separating the molecules of the ice to a greater distance, and so increasing its analogy with fluids.

The author concluded by stating, that he put forth this theory rather as a basis for future investigations than as an absolute truth. All the facts already known he considered to be capable of being brought under it, and the investigations at present in progress would very soon prove it correct or otherwise.

XXXIX. *Intelligence and Miscellaneous Articles.*

ON THE DISCOVERY OF NATIVE LEAD IN IRELAND. BY MR. T. AUSTIN, JUN.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

As the existence of lead in its native state has been deemed problematical by our most eminent mineralogists, it may not be un-

interesting to the readers of the Philosophical Magazine to state, that in the autumn of the year 1839, whilst engaged in a mineralogical examination of a part of the country in the neighbourhood of Kenmare, County Kerry, I discovered a few specimens of this rare mineral in the carboniferous limestone of that district; and more recently, when surveying in the vicinity of Bristol, I have succeeded in obtaining it in tolerable abundance from the same formation in several localities. It occurs either coating the faces of the minor joints, or filling up small crannies at the points where several joints intercept each other; in the latter situations the pieces sometimes weigh nearly half an ounce, in others it appears merely as a fine film.

This interesting mineral has been described as occurring in small masses in the lavas of Madeira and other volcanic districts, and also as existing under very dubious circumstances at Alston in Cumberland, in minute globules in the interior of small lumps of *slaggy* galena within reach of the surface.

Inclosed you will find specimens of the metal exactly in the state it was found.

I am, Gentlemen, your obliged Servant,

THOMAS AUSTIN, Jun.,
Mineral Surveyor.

1 Paul Street, Kingsdown, Bristol,
Dec. 31, 1842.

ON THE COMPOSITION OF PARAFFIN. BY M. LEWY.

Paraffin has already been subjected to the researches of various chemists; according to the analysis of M. Jules Gay-Lussac, its composition is the same as that of olefiant gas; its chemical equivalent has not hitherto been ascertained, because it does not form any compounds whatever.

M. Lewy states that his experiments were performed in the laboratory of M. Dumas, to whom he was indebted for the various specimens of paraffin employed in his analyses; some of them were prepared by M. Malaguti from the bituminous schistus of Autun, and from various kinds of wax. Some specimens of rough paraffin were purified by M. Lewy himself by repeated treatment with alcohol and æther, then distilling the products and again crystallizing them in ætherized alcohol.

The paraffin thus obtained was perfectly white, and had the form of pearly scales. Its density was 0·89, it fused at about $85\frac{1}{2}°$ Fahr.; it may be distilled without alteration, and its boiling point appears to be between 694° and 712° of Fahrenheit.

The mean of eight analyses gave as the composition of paraffin,—

Carbon	85·03	
Hydrogen	14·87	99·90.

This composition the author remarks does not agree with that of olefiant gas, and the simplest formula which can be deduced from it he states to be $C^{20} H^{42}$, which gives

Carbon	85·10	
Hydrogen	14·89	99·99.

With the intention of ascertaining the condensation of the elements of the equivalent of paraffin, M. Lewy attempted to determine its density in the state of vapour; this operation requires many precautions, for paraffin undergoes incipient decomposition at a temperature but little higher than that of its boiling point; it is difficult not to obtain some carburetted hydrogen gas, of which the eudiometric analysis must be performed in order to calculate with greater exactness the density according to the data of the experiment; when however the operation is carefully conducted, the paraffin remains white in the receiver, and analysis indicates no alteration in it. The results of three determinations did not sufficiently agree to admit of proving the equivalent of paraffin with certainty. The numbers oscillated between 10 and 11·8. All that can be stated with certainty is, that the molecule of paraffin contains at least 40 equivalents of carbon.

Taking the composition as $C^{40}\ H^{24}$, calculation will give the following numbers:—

$$\begin{array}{lcr} \text{40 vol. of vapour of carbon} & = & 33{\cdot}728 \\ \text{84 vol. of hydrogen} & = & 5{\cdot}779 \\ \hline & & \dfrac{39{\cdot}507}{4} = 9{\cdot}879. \end{array}$$

Assuming the formula $C^{48}\ H^{100}$, we shall have

$$\begin{array}{lcr} \text{40 vol. of vapour of carbon} & = & 40{\cdot}32 \\ \text{100 vol. of hydrogen} & = & 6{\cdot}88 \\ \hline & & \dfrac{42{\cdot}20}{4} = 11{\cdot}8 \end{array}$$

This formula gives

C^{48}	3600	85·20
H^{100}	625	14·78
	4225	99·98

numbers which agree equally well with the experiments.

M. Lewy remarks, that it is evident that the formula for paraffin requires to be verified by means less subject to error; he states also that he has made several attempts to obtain products derived from paraffin by the influence of several chemical agents; chlorine has a marked action, and yields under favourable circumstances a crystalline body which contains much chlorine.

An examination of the products derived from paraffin, as well as researches on wax and its relation to paraffin, will undoubtedly furnish new data for establishing the equivalent of paraffin.—*Ann. de Chemie,* Juillet 1842.

ANALYSIS OF HUMAN BONES. BY BERZELIUS AND BY MARCHAND.

The analysis of Berzelius has not we believe been very recently performed, but that of Marchand has; we give them both that they may be compared.

Berzelius.—Cartilage completely soluble in water	32·17
Vessels	1·13

Basic phosphate of lime, with a little fluoride of calcium	53·04
Carbonate of lime	11·30
Phosphate of magnesia	1·16
Soda, with a very small quantity of chloride of sodium	1·20
	100·

Marchand.—Cartilage insoluble in hydrochloric acid	27·23
Cartilage soluble in hydrochloric acid	5·02
Vessels	1·01
Basic phosphate of lime	52·26
Fluoride of calcium	1·00
Carbonate of lime	10·21
Phosphate of magnesia	1·05
Soda	0·92
Chloride of sodium	0·25
Oxides of iron and manganese, and loss	1·05
	100·

Journal de Pharmacie, Decembre 1842.

COMPOUNDS OF PHOSPHORIC ACID WITH OXIDE OF LEAD. BY M. WINCKLER.

According to the researches of Prof. Graham, phosphoric acid forms, under certain circumstances, three different hydrates; common phosphoric acid ($P^2 O^5$ 3 $H^2 O$), pyrophosphoric ($P^2 O^5$ 2 $H^2 O$) and metaphosphoric acid ($P^2 O^5 H^2 O$), the atoms of water of which may be replaced by a corresponding number of atoms of different basic oxides. It is indicated in the German translation of Graham's Chemistry by M. Otto, in what manner the formation of the salt of silver, whether uni- bi- or tribasic, is effected by employing the corresponding salts of soda and nitrate of silver. It is also stated at the same time, that nitrate of lead yields, in an analogous manner, corresponding salts of phosphate of lead. M. Winckler has verified the accuracy of this latter assertion. He obtained with the metaphosphate, pyrophosphate and phosphate of soda, the following series of compounds: Pb O $P^2 O^5$ (metaphosphate of lead), 2 Pb O $P^2 O^5$ (pyrophosphate of lead), 3 Pb O $P^2 O^5$ (phosphate of lead).—*Journal de Pharmacie,* Novembre 1842.

NEW METHOD OF PRECIPITATING METALS IN THE STATE OF SULPHURET. BY M. C. HIMLY.

This method consists in replacing, in the first operation, sulphuretted hydrogen, the use of which is not free from inconvenience, by the alkaline hyposulphites. Hyposulphurous acid may, in fact, be considered as formed of sulphurous acid and sulphur; it readily decomposes into these two substances, when separated from its salts by more powerful acids; a mixture is then formed, in which the sulphurous acid, on account of its great tendency to pass to a higher degree of oxidizement, at the expense of the oxygen of other substances, is capable of replacing the hydrogen of the sulphuretted hy-

drogen; whilst the sulphur, at the moment of its simultaneous separation, acts the same part as the sulphur of the sulphuretted hydrogen; it combines with the deoxidized radical to form a metallic sulphuret. The author cites some examples in support of his process: if to a solution of a neutral arseniate one of hyposulphite of soda be added in excess, the mixture may be heated to ebullition without producing any sensible change; but if hydrochloric acid be then added, the arsenic is immediately precipitated in the state of sulphuret. The decomposition is more slow at common temperatures; but by observing certain rules and precautions it may be rendered perfect. By means of this reagent results are obtained in a few minutes which would have required at least a day with a current of sulphuretted hydrogen. The decomposition of the salts of antimony and copper by hyposulphite of soda and hydrochloric acid, is neither less easy nor complete.

The application of the hyposulphite of soda, potash or ammonia to the quantitative separation of metals appears to be subject to certain principles, which may be thus stated: some metallic oxides dissolve readily on an alkaline hyposulphite, when a little excess of alkali has been added to it; other metallic oxides are insoluble in it; a great number of double salts are formed, which have not hitherto been examined. It is thus, for example, that chloride of platina and potassium dissolves very readily, when gently heated, in hyposulphite of soda; at a boiling heat there are produced much sulphuret of platina and free sulphuric acid; if the hyposulphite of soda be in excess, and hydrochloric acid be added to it, the platina is completely precipitated by heat.

It appears that the metals, which form soluble sulphur salts, are those which the hyposulphite of soda dissolves readily, and the dissolving action exerted by hydrosulphate of ammonia partially decomposed by the sulphuret of antimony, arsenic, &c., and which is incomparably more energetic than that of pure hydrosulphate of ammonia, may be attributed rather to the hyposulphite of ammonia which exists in it, than to a higher degree of sulphuration.

It may be added generally, that the metals which sulphuretted hydrogen is capable of precipitating from their solutions in an acid, may also be precipitated by the hyposulphites. There are, however, some peculiar exceptions, as cadmium and bismuth for example, which will admit of the separation of some metals of this group from each other. In the third place, those metals which sulphuretted hydrogen does not precipitate from solution in acids, are not precipitated by hyposulphurous acid.

As to the acidifiable metals, it is with regard to them that the reducing power of hyposulphurous acid acts with the greatest energy: but the experiments of the author have been hitherto confined to antimony and arsenic; he proposes however to continue his researches.—*Journal de Pharmacie*, Novembre 1842.

NEW SCIENTIFIC BOOKS.

Narrative of a Voyage round the World, performed in H. M. S. Sulphur, during the years 1836–1842. By Captain Sir Edward Belcher, R.N., F.R.A.S. Two volumes. Lond. 1843, 8vo.

Report on the Geology of the County of Londonderry and of parts of Tyrone and Fermanagh; examined and described under the authority of the Master-General and the Board of Ordnance. By J. E. Portlock, F.R.S., Capt. R. E. Dublin 1843, 8vo.

An Introduction to the Study of Chemical Philosophy. By J. F. Daniell, For. Sec. R. S., Prof. Chem. King's College. Lond. 1843, 8vo.

Proceedings of the Glasgow Philosophical Society, 1841–42. Glasgow and Lond. 1842, 8vo.

METEOROLOGICAL OBSERVATIONS FOR JAN. 1843.

Chiswick.—January 1. Clear and fine. 2, 3. Frosty: fine. 4. Rain: clear. 5. Clear: rain and sleet. 6. Frosty: overcast. 7, 8. Cloudy. 9. Clear and frosty. 10. Stormy and wet: very boisterous. 11. Clear and frosty: very fine. 12. Hazy: clear: hurricane at night. 13. Stormy and wet: very boisterous: barometer at noon exceedingly low. 14. Clear and windy: densely overcast: snow at night. 15. Cloudy: clear and frosty. 16. Cold and dry: fine. 17. Overcast. 18. Hazy: dense fog. 19. Dense fog. 20—22. Hazy. 23. Very fine: overcast: stormy, with rain at night. 24. Overcast. 25. Very fine. 26, 27. Cloudy. 28. Cloudy: clear and fine. 29. Overcast. 30. Very fine. 31. Uniformly overcast: stormy, with rain at night.—Mean temperature of the month 3° above the average. The barometer on the 13th was lower than it has been observed in the neighbourhood of London since 1821.

Boston.—Jan. 1—3. Fine. 4. Rain. 5. Cloudy: rain and snow early A.M. 6. Fine. 7. Cloudy. 8. Cloudy: rain early A.M. 9. Fine. 10. Windy: rain early A.M.: stormy P.M., with snow. 11. Windy. 12. Cloudy: rain P.M. 13. Stormy: rain early A.M. (barometer 2 P.M. 27°·80): rain P.M.: stormy night. 14. Stormy: snow P.M. 15—18. Cloudy. 19. Foggy. 20—23. Cloudy. 24. Cloudy: rain early A.M. 25. Fine. 26. Cloudy: rain early A.M. 27. Cloudy. 28. Windy. 29. Cloudy. 30. Cloudy: stormy P.M. 31. Fine.

Sandwick Manse, Orkney.—Jan. 1. Hail-showers. 2. Snow. 3. Cloudy. 4. Showers: large hail—broke windows. 5. Hail-showers. 6. Showers. 7. Hail-showers—broke windows: thunder and lightning. 8. Snow-showers and hail. 9. Snow-showers: rain. 10. Snow-showers. 11. Snowing at noon: clear at night. 12. Clear: frost and snow. 13. Snow: thaw-showers. 14. Frost: thaw-showers. 15. Showers. 16. Showers: cloudy. 17. Drizzle. 18. Cloudy: drizzle. 19. Showers: clear. 20—22. Clear. 23. Clear: drops. 24. Cloudy. 25. Cloudy: drizzle. 26. Clear: rain. 27. Rain: showers. 28. Drizzle: showers. 29—31. Showers.

Applegarth Manse, Dumfries-shire.—Jan. 1. Frost A.M.: shower: frost P.M. 2. Frost A.M.: frost P.M. 3. Showers. 4. Snow and rain. 5. Frost: high wind. 6. Drizzling rain. 7. Rain and wind. 8. Snow: frost. 9. Snow: rain: wind. 10. Snow: frost. 11. Frost: lunar halo. 12. Hard frost. 13—15. Frost: drifting snow. 16. Thaw A.M.: frost P.M. 17, 18. Thaw: rain. 19. Thick fog and thaw. 20. Fair, but cloudy. 21. Fair A.M.: drizzly P.M. 22. Rain early A.M.: cleared. 23. Rain. 24. Fair, but misty. 25. Rain P.M. 26. Wet A.M. 27. Wet P.M. 28. Storm of wind and rain. 29. Wet and stormy. 30. Heavy showers. 31. Rain.

Meteorological Observations made at the Apartments of the Royal Society, LONDON, *by the Assistant Secretary*, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London; by* Mr. Veall, *at* BOSTON; *by the* Rev. W. Dunbar, *at Applegarth Manse* DUMFRIES-SHIRE; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month. 1843. Jan.	Barometer. London: Roy. Soc. 9 a.m.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 9 p.m.	Barometer. Orkney, Sandwick. 9¼ a.m.	Barometer. Orkney, Sandwick. 8½ p.m.	Thermometer. London: R.S. Fahr. 9 a.m.	Thermometer. London: R.S. Self-reg. Mx.	Thermometer. London: R.S. Self-reg. Mn.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ a.m.	Thermometer. Dumf.-shire. Max.	Thermometer. Dumf.-shire. Min.	Thermometer. Orkney, Sandwick. 9¼ a.m.	Thermometer. Orkney, Sandwick. 8½ p.m.
1.	30·298	30·257	30·131	29·85	30·14	30·03	30·00	30·01	38·2	55·7	38·5	41	25	33·5	39	27½	34	33
2.	30·060	30·086	30·017	29·67	29·97	30·01	30·09	30·11	36·0	39·8	33·6	38	19	32	38	30	35	34
3.	30·226	30·205	30·107	29·87	30·00	29·74	29·83	29·53	30·7	38·3	29·8	43	27	27·5	45	21	35	45
4.	29·888	29·977	29·884	29·49	29·63	29·60	29·36	29·22	41·3	42·2	30·5	43	21	41	49	31	37½	38
5.	29·836	30·020	29·772	29·38	29·65	29·94	29·61	29·93	38·8	44·3	35·6	31	31	36	38	31	36	38
6.	30·134	30·080	29·997	29·75	29·92	29·74	29·61	29·53	36·7	42·5	34·7	41	34	35	46	32	39	39
7.	29·946	29·917	29·589	29·53	29·51	29·20	29·17	28·91	41·8	44·9	37·0	46	36	40	44	41	34	34
☽ 8.	29·388	29·377	29·311	29·03	28·98	29·16	28·75	29·11	40·2	46·3	40·3	42	30	36	38½	30	34½	33
9.	29·684	29·659	29·206	29·30	29·37	28·79	29·05	28·42	34·5	41·4	32·8	47	32	31	43½	23½	34	39
10.	28·892	29·206	29·068	28·49	28·47	28·70	28·32	28·48	36·8	47·0	34·8	42	30	36·5	40½	31	34½	37½
11.	29·111	29·108	28·796	28·71	28·78	28·74	28·76	28·79	34·3	43·0	34·2	40	31	33	36	28	37	33½
12.	28·738	29·120	28·706	28·56	28·73	28·79	28·81	28·82	32·5	39·6	33·0	43	23	24	32½	20	32	25
13.	28·354	28·649	28·181	28·10	28·00	28·03	28·57	28·51	41·0	42·8	31·2	46	37	36	36	18	34	34½
14.	29·046	29·030	28·700	28·54	28·59	28·63	28·52	28·51	36·8	45·4	36·7	39	28	37	35	31½	34	36
15.	28·922	29·070	28·826	28·63	28·63	28·90	28·62	28·94	34·5	41·0	32·2	40	26	27	34	20½	37	39
○ 16.	29·354	29·942	29·310	29·06	29·38	29·85	29·62	29·90	38·3	39·0	31·0	41	30	36	41	29	39½	37
17.	30·214	30·198	30·182	29·83	29·88	29·92	29·61	29·69	36·8	41·7	35·6	42	37	34	43	28	44	47
18.	30·366	30·454	30·326	29·95	30·06	30·19	29·84	30·02	43·4	44·4	37·0	49	33	42	46½	39	48½	47½
19.	30·538	30·502	30·438	30·10	30·25	30·28	30·16	30·18	36·8	49·4	36·8	44	34	41	45	41½	36	44
20.	30·332	30·237	30·086	30·00	30·13	29·98	30·11	30·00	38·3	44·7	36·6	39	29	41·5	44	40	42	39
21.	30·032	30·030	30·008	29·64	29·78	29·75	29·86	29·71	33·8	40·4	34·2	41	34	37	37½	29	40	38
22.	30·060	30·038	30·003	29·74	29·71	29·75	29·71	29·69	40·8	41·8	34·2	44	34	40·5	47	33½	41	45
☾ 23.	30·062	30·027	29·892	29·63	29·69	29·40	29·62	29·43	42·7	44·0	41·2	48	43	41·5	45½	37	41	43
24.	29·896	29·869	29·837	29·42	29·41	29·50	29·31	29·30	45·3	47·0	42·4	48	39	45	47	43	43½	43½
25.	30·046	30·059	29·999	29·55	29·65	29·70	29·42	29·66	42·6	48·0	42·8	49	44	39	47	41	46	46
26.	30·010	30·038	30·010	29·50	29·74	29·72	29·78	29·40	47·7	49·6	42·7	51	45	48	48	13	45	44
27.	29·992	29·959	29·901	29·44	29·59	29·48	29·12	29·06	50·7	52·0	48·0	53	50	50	53	43	48	49
28.	29·804	29·907	29·772	29·16	29·36	29·60	28·96	29·37	54·8	56·2	51·0	56	45	53	52	45	46	41½
29.	29·954	29·911	29·826	29·38	29·50	29·40	29·39	29·12	49·4	57·7	47·3	55	48	46	52	10½	43	47
● 30.	29·768	29·905	29·724	29·16	29·28	29·56	28·85	29·42	51·3	55·3	49·7	55	36	51	50	45	45	42½
31.	29·964	29·931	29·834	29·46	29·56	29·30	29·24	28·89	45·3	49·8	42·3	51	41	40	50	39½	45	45
Mean.	29·772	29·831	29·659	29·35	29·462	29·470	29·346	29·343	40·4	45·7	37·7	44·15	34·03	38·4	43·3	33·3	39·38	39·91

Days of Month. 1843. Jan.	Wind. London: R.S. 9 a.m.	Wind. Chiswick. 1 p.m.	Wind. Boston.	Wind. Dumfries-shire.	Wind. Orkney, Sandwick.	Rain. London: R.S. 9 a.m.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Rain. Orkney, Sandwick.	Dew-point. Lond. R.S. 9 a.m.
1.	nw.	n.	calm	w.	nw.	·013	......	......	......	·06	38
2.	w.	nw.	calm	w.	nw.	......	......	......	......	·11	35
3.	nw.	sw.	calm	s.	s.	......	......	......	......	......	30
4.	s.	w.	w.	sw.	w.	......	·12	·15	......	·31	37
5.	w.	w.	calm	nw.	nw.	·152	·06	·18	......	·26	34
6.	w.	w.	calm	w.	sw.	......	·01	......	......	·04	32
7.	ssw.	w.	calm	sw.	sw.	·030	·14	......	......	·41	37
☽ 8.	sw.	sw.	calm	w.	w.	·163	......	·07	1·00	·17	38
9.	w.	sw.	calm	sw.	s.	......	·09	......	......	·26	32
10.	w.	w.	w.	nw.	nw.	·133	·06	·04	......	·92	37
11.	w.	sw.	w.	wsw.	nw.	......	·16	......	......	·07	33
12.	n var.	n.	calm	e.	se.	·088	·29	......	......	......	31
13.	s.	sw.	s.	c & w.	e.	·223	·04	·26	......	......	37
14.	w var.	sw.	w.	w.	nw.	......	·16	·20	......	·54	31
15.	s.	w.	calm	nnw.	nw.	·130	......	......	......	·71	32
○ 16.	w.	nw.	ne	nw.	n.	......	......	......	0·80	·17	32
17.	nw.	sw.	calm	nw.	sw.	......	·03	......	......	·06	32
18.	w.	sw.	calm	sw& e.	sw.	·052	......	......	......	·04	38
19.	w.	s.	calm	s.	sw.	......	......	......	......	·25	36
20.	e.	e.	calm	e.	ssw.	......	·04	......	......	......	37
21.	e.	s.	calm	e.	se.	......	......	......	......	......	34
22.	s.	sw.	calm	ese.	s.	......	·02	......	......	......	36
☾ 23.	s.	s.	calm	s.	s.	......	·05	......	0·11	......	39
24.	s.	sw.	calm	sw.	s.	·027	·02	·36	......	·06	41
25.	e.	sw.	calm	sw.	w.	......	......	......	......	·02	41
26.	s.	sw.	w.	sw.	w.	......	......	·08	......	·16	44
27.	s.	sw.	calm	w.	sw	......	......	......	......	·51	48
28.	s.	w.	w.	w.	w.	......	·02	......	......	·18	50
29.	sw.	w.	calm	wnw.	w.	......	......	......	......	·39	48
● 30.	w.	w.	w.	nw.	w.	......	......	......	1·11	·39	49
31.	s.	sw.	w.	ssw.	s.	......	·07	......	......	·16	44
Mean.						Sum. 1·011	1·38	1·34	3·02	6·25	Mean. 38

THE

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[THIRD SERIES.]

APRIL 1843.

XL. *On Apparatus for the circular Polarization of Light in Liquids. By the Rev.* BADEN POWELL, *M.A., F.R.S., &c., Savilian Professor of Geometry, Oxford*.*

THE subject of circular polarization developed in light in passing through certain substances, especially liquids, is one which has at the present day attracted considerable attention, not merely among those who study the properties of light as such, but also among *chemists*, as supplying a test of the existence of certain elements in the solutions which exhibit it. It has thus become an object of importance to devise means for facilitating the performance of the observations by which the existence of this property is detected and its amount measured.

The general nature of all methods and apparatus for this purpose must be obvious on the first conception of the property itself. But they may admit of considerable modifications in their details, so as to be more or less applicable under different conditions.

The most ample details as to the methods of experimenting have been given by the original discoverer of their singular phænomena, M. Biot, in several memoirs in the *Annales de Chimie* †; he has also devised a most complete and accurate apparatus for the purpose, which may be procured from Paris made under his direction. Without in the slightest degree meaning to disparage the excellence of that apparatus, or indeed the necessity for it in all refined and accurate researches, yet for the more general purposes of the chemical inquirer, and especially for students, it must be allowed that its complexity, difficulty of adjustment, and expense must stand much

* Communicated by the Author.

[† See also Taylor's Scientific Memoirs, vol. i. p. 584 and p. 600. ED.]

in the way of its general introduction. Under this impression, derived in some measure from my own experience, it occurred to me to examine in what respects it might be capable of simplification.

Following up this idea, after many trials I succeeded in contriving an apparatus, which, at least for all general objects, answers the purpose, and is of extremely simple, easy, and cheap construction; requiring in fact little more materials for its several parts than are either to be found already in the laboratory of every chemist, or may be readily procured, or constructed by the most ordinary workman. Of such a contrivance I gave a short account to the Chemical Section of the British Association at the Manchester Meeting, 1842, and a short description with a sketch representing the *principle*, is given in the reports of the Association for that year.

It has appeared desirable however to offer, to those interested in the subject, some further details, as many may wish to be able to construct such an apparatus for themselves; and still more, as there are one or two improvements in the details which have suggested themselves since I gave the description just referred to. I proceed therefore to describe more precisely the principle of the construction as well as its details.

In the construction of M. Biot, the object viewed is the round disc of polarized light transmitted through the aperture of the diaphragm in the tube of the polarizer; in order to see this distinctly through a considerable thickness of liquid, it is indispensable that the liquid be contained in a tube having parallel ends of plate-glass. Such a tube of course requires to be constructed accurately; the necessary supply of them of different lengths, and for comparative experiments, &c., is expensive; and there is a considerable difficulty in filling them, and fitting on the glass ends so as to exclude air-bubbles, &c. Again, the double-refracting prism, which is necessary to procure a separation of the two images, must be of the most accurate construction, so as to give images absolutely free from colour from refraction, in order to distinguish precisely the tints developed by the polarization.

The main principle of my construction refers to both these sources of difficulty: with respect to the first, I employ common test tubes without any other mode of termination than that furnished by the rounded bottom of the tube, however irregular, and the level surface of the liquid at the top, the tube being necessarily placed in a vertical position. Through such a tube however the image of the disc of polarized light will obviously be very irregular, even if the liquid be perfectly transparent; and no distinct image is seen if it be only

semi-transparent; so that the double refracting prism cannot be applied to the analysis of it. But this difficulty is at once provided for, and the compound achromatized prism dispensed with, by the very simple eye-piece which I have adopted; for the use of which the fluid need not be absolutely transparent if it only allows a sufficient *quantity* of light to pass.

This eye-piece consists simply of a rhomb of calc spar in its natural state, the light being admitted through a small hole in the end or bottom of the short tube which contains it, of such a size, relatively to the thickness of the crystal which the light traverses, that the two emergent images of the hole shall not overlap each other; this is easily found by trial; as however these images may be too small readily to follow the changes of tint in them, I magnify them by a lens of short focus fitted in a short tube sliding in the upper part of that containing the rhomb. It only remains to attach to the tube a graduated rim which can turn with it in an outer cell which is attached to the stand of the apparatus, and on which changes in azimuth, or the *arcs of rotation* of the rhomb about the ray, are measured.

By the arc necessary to be revolved through, by the rhomb, in order to make the extraordinary image come to its minimum, as compared with the position for that effect, when no liquid is interposed, it is, that we estimate the rotatory power of the liquid.

In the eye-piece thus constructed, it will be evident that the object at which we look is the small hole at the bottom of the rhomb. So long then as *enough light* enters that hole it is immaterial how irregular the refractions of it may be in passing through the tube before it reaches the hole: we are independent of the distinctness of the image which it transmits, and the only material point is the *intensity* of light which the liquid allows to pass; and this is in fact one of the chief difficulties we have to contend with in these experiments; since many solutions which appear sufficiently transparent in small thicknesses are by no means so when we come to thicknesses of 12 inches or more.

In my first construction the polarizing part of the apparatus consisted of a simple plate of glass inclined $35\frac{1}{2}°$ to the axis of the tube, and in order to have the polarized ray vertical, it was necessary to throw the light on this reflector by means of a small silvered mirror.

In this part, however, I find it a most material improvement to substitute a *Nicol prism* for the plate of glass; this of course has its axis coincident with that of the tube, and the small

silvered mirror is placed beneath it, supported in such a manner as to be easily inclined, so as to throw the light in a proper direction. The great advantage gained is the saving of light, of which of course much is lost in the reflexion at the glass: and as a considerable number of the solutions we wish to examine are but imperfectly transparent, this saving becomes important.

In order to determine the *direction* of rotation, or the right or left-handedness of the polarization, it is necessary to compare two different thicknesses of the liquid: in other instances it is interesting to compare the effects of two different liquids: for these purposes it is extremely convenient to have a contrivance by which two tubes can be brought in succession into the apparatus without deranging the adjustment of the other parts.

For different fluids different lengths of tube are required; for some highly energetic, as oil of turpentine, with a length of 5 or 6 inches tints are seen; with most, solutions of sugar, &c., from 12 to 18 or even 24 inches are necessary. Hence the apparatus should be so contrived as to admit of the eyepiece being slid up and down as occasion requires.

All these conditions are fulfilled in the construction of which I annex a sketch for the convenience of those who may wish to construct similar ones; and of which the following few details will abundantly suffice for explanation.

Fig. 1. gives a general perspective view of the whole in its latest form. Fig. 2. is the lower part according to the first construction. (P) is the polarizing part, (A) the analysing.

In each of these figures (*s*) is the silvered mirror which first receives the ray (either from a flame or the sky), which is thence thrown on the polarizer (*p*), which in fig. 1 is a Nicol prism fitted into the hole (*q*) in fig. 2, a plain glass reflector inclined $35\frac{1}{2}°$ to the axis, the mirror (*s*) being capable of inclination to bring the ray into the proper direction. In fig. 1. the course of the ray is represented by the dotted line, when no medium is interposed, from its first reflexion, through the analyser, to the eye (*e*). A section of the analyser is given in fig. 3, in which (*r*) is the rhomb, (*l*) the lens, and (*k*) the small hole in the bottom of the short tube or box containing the rhomb: (*m*) is the section of the circular rim with verniers at the openings (*v*, *v*), through which the graduation on the circle (*n*) beneath is read. The circle (*m*) is moved round with the tube by means of its milled edge: (*n*) is fixed to the stand. The arm of the stand (H) which carries (A) should be capable of moving up and down according to the height of the tubes employed, and of being fixed by a clamping

Fig. 1.

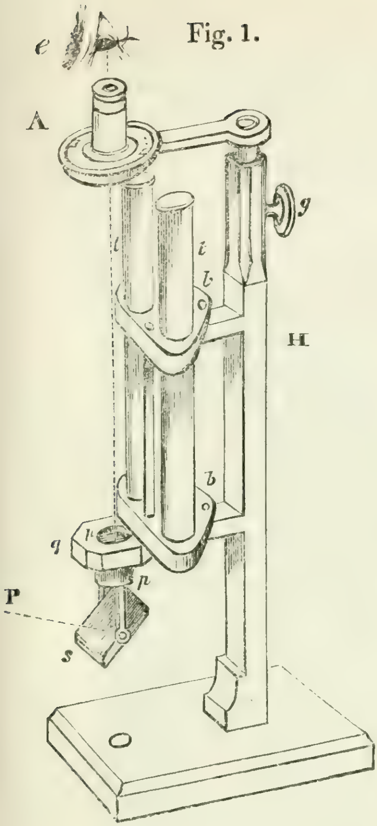


Fig. 2.

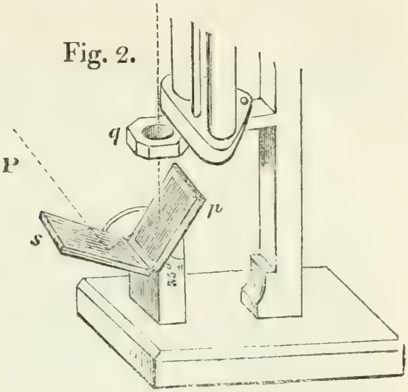


Fig. 3.

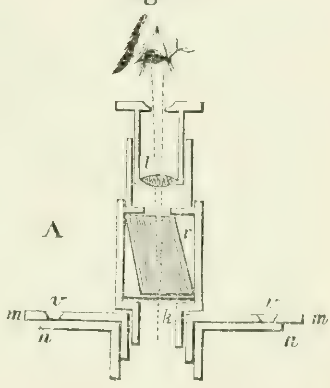
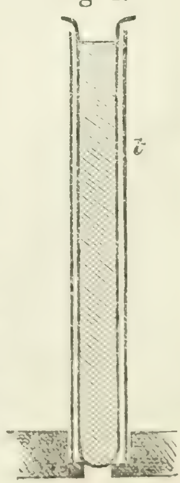


Fig. 4.



screw (*g*). The part carrying the tubes (*t t*) consists of a frame holding them in a vertical position, each tube being inclosed in an opake case. The whole frame with the tubes is made to turn about the pivots (*b b*), so that either tube in succession can be brought over the hole (*q*), and with its axis in the line of the ray. Fig. 4 is a section of one of the tubes in its opake covering, to show the manner of its fitting into the frame at the bottom, in which the opening is cut so as just to allow the tube to rest against its edges, and to leave an opening as large as the area of the polarizer.

XLI. *On the Action of the Rays of the Solar Spectrum on Vegetable Colours, and on some new Photographic Processes.* *By* Sir John F. W. Herschel, *Bart., K.H., F.R.S.*

[Continued from p. 180, and concluded.]

Postscript added August 29, 1842.

217. I GLADLY avail myself of the permission accorded by the President and Council to append to this communication, in the form of a Postscript, some additional facts illustrative of the singular properties of iron as a photographic ingredient, which have been partially developed in the latter articles of it, as well as an account of some highly interesting photographic processes dependent on those properties, which the superb weather we have lately enjoyed has enabled me to discover, as also to describe a better method of fixing the picture, in the process to which I have given the name of *Chrysotype*; that described in Art. 212 proving insufficient. The new method (in which the hydriodate is substituted for the hydrobromate of potash) proves perfectly effectual; pictures fixed by it not having suffered in the smallest degree, either from long exposure to sunshine, or from keeping; alone, or in contact with other papers. It is as follows:—As soon as the picture is satisfactorily brought out by the auriferous liquid (Art. 212.) it is to be rinsed in spring water, which must be three times renewed, letting it remain in the third water five or ten minutes. It is then to be blotted off and dried, after which it is to be washed on both sides with a somewhat weak solution of hydriodate of potash. If there be any free chloride of gold present in the pores of the paper, it will be discoloured, the lights passing to a ruddy brown; but they speedily whiten again spontaneously, or at all events, on throwing it (after lying a minute or two) into fresh water, in which, being again rinsed and dried, it is now perfectly fixed.

218. If paper prepared as above recommended for the chrysotype, either with the ammonio-citrate or ammonio-tartrate of iron, and impressed, as in that process, with a latent picture, be washed with nitrate of silver instead of a solution of gold, a very sharp and beautiful picture is developed, of great intensity. Its disclosure is not instantaneous; a few moments elapse without apparent effect; the dark shades are then first touched in, and by degrees the details appear, but much more slowly than in the case of gold. In two or three minutes, however, the maximum of distinctness will not fail to be attained. The

picture may be fixed by the hyposulphite of soda, which alone, I believe, can be fully depended on for fixing argentine photographs.

219. *Cyanotype.*—If a nomenclature of this kind be admitted (and it has some recommendations), the whole class of processes in which cyanogen in its combinations with iron performs a leading part, and in which the resulting pictures are blue, may be designated by this epithet. The varieties of cyanotype processes seem to be innumerable, but that which I shall now describe deserves particular notice, not only for its pre-eminent beauty while in progress, but as illustrating the peculiar power of the ammoniacal and other persalts of iron above mentioned to receive a latent picture, susceptible of development by a great variety of stimuli. This process consists in simply passing over the ammonio-citrated paper on which such a latent picture has been impressed, *very sparingly and evenly*, a wash of the solution of the common yellow ferrocyanate (prussiate) of potash *. The latent picture, if not so faint as to be quite invisible (and for this purpose it should not be so), is negative. As soon as the liquid is applied, which cannot be in too thin a film, the negative picture vanishes, and by very slow degrees is replaced by a positive one of a violet-blue colour on a greenish yellow ground, which at a certain moment possesses a high degree of sharpness and singular beauty and delicacy of tint. If at this instant it be thrown into water, it passes immediately to Prussian blue, losing at the same time, however, much of its sharpness, and sometimes indeed becoming quite blotty and confused. But if this be delayed, the picture, after attaining a certain maximum of distinctness, grows rapidly confused, especially if the quantity of liquid applied be more than the paper can easily and completely absorb, or if the brush in applying it be allowed to rest on, or be passed twice over any part. The effect then becomes that of a coarse and ill-printed wood-cut, all the strong shades being run together, and a total absence prevailing of half lights.

220. To prevent this confusion, gum-arabic may be added

* Vulgarly, and in my opinion very conveniently and *correctly* so called, according to the true intent and meaning of Scheele. Trivial names for common objects are to be maintained and defended on principles far more general than systematic nomenclature. For this reason I trust never to see the name muriatic give way to hydrochloric, or nitric thrust aside for azotic acid. The *prussic acid* is that acid, whatever it be, which, united with oxide of iron as a base, forms *Prussian blue*, from which remarkable compound the whole history of cyanogen originated. The now ascertained existence of another ferrocyanate makes this recurrence to a trivial name for the vulgar one more necessary.

to the prussiated solution, by which it is hindered from spreading unmanageably within the pores of the paper, and the precipitated Prussian blue allowed time to agglomerate and fix itself on the fibres. By the use of this ingredient also, a much thinner and more equable film may be spread over the surface; and *when perfectly dry*, if not sufficiently developed, the application may be repeated. By operating thus I have occasionally (though rarely) succeeded in producing pictures of great beauty and richness of effect, which they retain (if not thrown into water) between the leaves of a portfolio, and have even a certain degree of fixity—fading in a strong light and recovering their tone in the dark. The manipulations of this process are, however, delicate, and complete success is comparatively rare.

221. If sulphocyanate of potash be added to the ammonio-citrate or ammonio-tartrate of iron, the peculiar red colour which that test induces on persalts of the metal is not produced, but appears at once on adding a drop or two of dilute sulphuric or nitric acid. This circumstance, joined to the perfect neutrality of these salts, and their power, in such neutral solution, of enduring, undecomposed, a boiling heat, contrary to the usual habitudes of the peroxide of iron *, together with their singular transformation by the action of light to proto-salts, in apparent opposition to a very strong affinity, has, I confess, inclined me to speculate on the possibility of their ferruginous base existing in them, not in the ordinary form of peroxide, but in one isomeric with it. The non-formation of Prussian blue, when their solutions are mixed with prussiate of potash (Art. 209), and the formation in its place of a deep violet-coloured liquid of singular instability under the action of light, seems to favour this idea. Nor is it altogether impossible that the peculiar "prepared" state superficially assumed by iron under the influence of nitric acid, first noticed by Keir, and since made the subject of experiment by M. Schönbein and myself†, may depend on a change superficially operated on the *iron itself* into a new metallic body isomeric with iron, unoxidable by nitric acid, and which may be considered as the radical of that peroxide which exists in the salts in question, and possibly also of an isomeric protoxide. A combination of the common protoxide with the isomeric peroxide, rather than with the same metal in a simply higher stage of oxidation, would afford a not un-

* See my paper on this subject in Philosophical Transactions, cxi. p. 293, [or Phil. Mag., S. 1. vol. lix. p. 86.—Edit.].

† See *Annales de Chimie*, tom. liv. p. 87, [or Phil. Mag., S. 3. vol. xi. p. 329.—Edit.].

plausible notion of the chemical nature of that peculiar intermediate oxide to which the name of "Ferroso-ferric" has been given by Berzelius. If (to render my meaning more clear) we for a moment consent to designate such an isomeric form of iron by the name siderium, the oxide in question might be regarded as a sideriate of iron. Both phosphorus and arsenic (bodies remarkable for sesqui-combinations) admit isomeric forms in their oxides and acids *. But to return from this digression.

222. If to a mixture of ammonio-citrate of iron and sulphocyanate of potash a small dose of nitric acid be added, the resulting red liquid spread on paper spontaneously whitens in the dark. If more acid be added till the point is attained when the discoloration begins to relax, and the paper when dry retains a considerable degree of colour, it is powerfully affected by light, and receives a positive picture with great rapidity, which, like the guaiacum impression noticed in Art. 154, appears at the back of the paper with even more distinctness than on its face. The impression, however, is pallid; fades on keeping, nor am I acquainted at present with any mode of fixing it.

223. If paper be washed with a mixture of the solutions of ammonio-citrate of iron and ferrosesquicyanate of potash, so as to contain the two salts in about equal proportions, and being then impressed with a picture, be thrown into water and dried, a negative blue picture will be produced agreeably to what is stated in Art. 154. This picture I have found to be susceptible of a very curious transformation, preceded by total obliteration. To effect this it must be washed with solution of proto-nitrate of mercury, which in a little time entirely discharges it. The nitrate being thoroughly washed out and the picture dried, a smooth iron is to be passed over it, somewhat hotter than is used for ironing linen, but not sufficiently so to scorch or injure the paper. The obliterated picture immediately reappears, not blue, but brown. If kept for some weeks in this state between the leaves of a portfolio, in complete darkness, it fades, and at length almost entirely disappears. But what is very singular, a fresh application of the heat revives and restores it to its full intensity.

224. This curious transformation is instructive in another way. It is not operated by light, at least not by light alone. *A certain temperature* must be attained, and that temperature suffices in total darkness. Nevertheless, I find that on exposing to a very concentrated spectrum (collected by a lens

* The latter from the late experiments and remarks of Rose on the vitreous state of the arsenious acid and its luminosity in crystallizing from acid solutions. [See Phil. Mag., S. 3. vol. vii. p. 534.—Edit.]

of short focus) a slip of paper duly prepared as above (that is to say, by washing with the mixed solutions, exposure to sunshine, washing, and discharging the uniform blue colour so induced as in the last article), its whiteness is changed to brown over the whole region of the red and orange rays, *but not beyond* the luminous spectrum. Three conclusions seem unavoidable;—1st, that it is the heat of these rays, not their light, which operates the change; 2ndly, that this heat possesses a peculiar chemical quality which is not possessed by the purely calorific rays outside of the visible spectrum, though far more intense; and, 3rdly, that the heat radiated from obscurely hot iron, abounds especially in rays analogous to those of the region of the spectrum above indicated. And there are the very same conclusions derived from the experiments on guaiacum in Art. 158—160.

225. Whatever be the state of the iron in the double salts in question, its reduction by blue light to the state of protoxide is indicated by many other reagents. If, for example, a slip of paper, prepared with the ammonio-citrate and partially sunned, be washed, when withdrawn, with bichromate of potash, the bichromate is deoxidized and precipitated on the sunned portion, just as it would be if directly exposed to the sun's rays. Every reagent in short which is susceptible of being deoxidated, wholly or in part, by contact with the protoxide of iron, is so also by contact with the sunned paper. Taking advantage of this property, I have been enabled to add another and very powerful element to the list of photographic ingredients.

226. *Photographic Properties of Mercury.*—This element is mercury. As an agent in the Daguerreotype process, it is not, strictly speaking, photographically affected. It operates there only in virtue of its readiness to amalgamate with silver, properly prepared to receive it. That it possesses *direct* photographic susceptibility, however, in a very eminent degree, is proved by the following experiment. Let a paper be washed over with a weak solution of periodide of iron, and when dry with a solution of proto-nitrate of mercury. A bright yellow paper is produced, which (if the right strength of the liquids be hit) is exceedingly sensitive while wet, darkening to a brown colour in a very few seconds in the sunshine. Withdrawn, the impression fades rapidly, and the paper in a few hours recovers its original colour. In operating this change of colour the whole spectrum is effective, with the exception of the thermic rays beyond the red.

227. Proto-nitrate of mercury simply washed over paper is slowly and feebly blackened by exposure to sunshine. And

if paper be impregnated with the ammonio-citrate, already so often mentioned, partially sunned, and then washed with the proto-nitrate, a reduction of the latter salt, and consequently blackening of the paper, takes place very slowly in the dark over the sunned portion, to nearly the same amount as in the direct action of the light on the simply nitrated paper.

228. But if the mercurial salt be subjected to the action of light in contact with the ammonio-citrate, or tartrate, the effect is far more powerful. Considering, at present, only the citric double-salt, a paper prepared by washing first with that salt and then with the mercurial proto-nitrate (drying between) is endowed with considerable sensibility, and darkness to a very deep brown, nay to complete blackness, on a moderate exposure to good sun. Very sharp and intense photographs of a negative character may be thus taken. They are however difficult to *fix*. The only method which I have found at all to succeed, has been by washing them with bichromate of potash and soaking them for twenty-four hours in water, which *dissolves out* the chromate of mercury for the most part, leaving however a yellow tint on the ground, which resists obstinately. But though pretty effectually fixed in this way against *light*, they are not so against *time*, as they fade considerably on keeping.

229. When the proto-nitrate of mercury is mixed, in solution, with either of the ammoniacal double salts, it forms a precipitate, which, worked up with a brush to the consistency of cream, is easily (and with certain precautions of manipulation*) very evenly spread on paper, producing photographic tablets of every variety of sensibility and inertness, according to the proportion of the doses used. By combining all three of the ingredients, and adding a small quantity of tartaric acid †, a paper is produced of a pretty high degree of sensibility (more than by the use of either separately), which in about half an hour or an hour, according to the sun, affords pictures of such force and depth of colour, such velvety richness of material, and such perfection of detail and preservation of the relative intensities of the light, as infinitely to sur-

* The cream should be spread as rapidly as possible over the whole paper, well worked in, cleared off as much as possible, and finished with a brush nearly dry, spread out broad and pressed to a straight thin edge, which must be drawn as lightly and evenly as possible over every part of the paper till the surface appears free from every streak, and barely moist.

† One measure of a solution of ammonio-citrate, and one of a solution of ammonio-tartrate of iron, containing, each, one-tenth of its weight of the respective salts. Tartaric acid, saturated solution, one-eighth of the *joint volumes* of the other solutions. Form a cream by pouring in as rapidly as possible one measure of a saturated solution of the proto-nitrate and well mixing with a brush.

pass any photographic production I have yet seen, and which indeed it seems impossible to go beyond. Most unfortunately, they cannot be preserved. Every attempt to fix them has resulted in the destruction of their beauty and force; and even when kept from light, they fade with more or less rapidity, some disappearing almost entirely in three or four days, while others have resisted tolerably well for a fortnight, or even a month. It is to an over-dose of tartaric acid that their more rapid deterioration seems to be due, and of course it is important to keep down the proportion of this ingredient as low as possible. But without it I have never succeeded in producing that peculiar velvety aspect on which the charm of these pictures chiefly depends, nor anything like the same intensity of colour without over-sunning.

230. I might here describe many other curious and interesting photographic results to which, under the genial influence of such a summer as, possibly, has never before been witnessed in England, I have been conducted. But in so doing I should surpass the reasonable bounds of a Postscript illustrative of my text, and abuse the privilege accorded me. Yet I cannot forbear noticing one at least, in which a line or dot engraving of any degree of delicacy is imitated, line for line, and dot for dot, in a manner which might deceive any but a practised artist to the point of rendering him unable to declare that the photograph had *not* been struck off from the original plate with common printing ink, by the ordinary process of copper-plate printing. The details of this process, which are delicate and somewhat tedious, cannot properly be stated here; if for no other reason, because I have not yet obtained a complete command over the result: but a microscopic examination of the specimens placed in the hands of our worthy Secretary, though somewhat marred by the accidents of manipulation, will I think suffice to justify the terms employed above.

XLII. *New Criteria for the Imaginary Roots of Equations. By* J. R. Young, *Esq., Professor of Mathematics in Belfast College.*

[Continued from p. 188 and concluded.]

IT is easy to see that the foregoing criteria furnish the rules proposed by Newton, in the Universal Arithmetic, for detecting imaginary roots in an equation. These rules have not, I believe, as yet been demonstrated; although, on account of their obvious utility and ready application, a rigorous proof of their truth has frequently been sought. The earliest dis-

cussion of Newton's rules is that of Maclaurin, in the Philosophical Transactions, vol. xxxvi. No. 408, who has entered into a very long and elaborate investigation of them, the results of which, however, only go the length of showing that "*some* imaginary roots exist in an equation," whenever any of Newton's criteria have place; and do not embrace the more general affirmation of the rules, that there are as many pairs of such roots as there are distinct criteria fulfilled.

It is no doubt from the misgiving and uncertainty always attendant upon an undemonstrated principle, however numerous the individual instances in which it may have been safely trusted, that these rules of Newton have fallen into neglect, in the analysis of equations. It is one object of the present paper to revive and demonstrate them: another, and the more immediate one, is to prove the adequacy of the criteria already given to determine the true character of a pair of doubtful roots, as soon as by actual development we have reached the point where, if they are real, the separation of them must take place.

It is desirable, when this stage of the approximation is arrived at, that we should be enabled to pronounce at once upon the nature of the roots under examination, from the conditions necessarily impressed upon the transformed coefficients thus attained, without having to apply to additional tests, or to execute any new transformations or by-operations, for this purpose, as in the methods hitherto proposed. This object may be effected from the following considerations.

I have elsewhere shown (Theory of Equations, p. 263), that when two roots, differing but little from equality, or concurring in several leading figures, are to be developed, these figures, after a certain early stage, will be furnished, one after another, by either of the two concurrent expressions which, in the arrangement below, stand vertically under the functions into which these roots first enter:—

$$\begin{array}{cccc}
f_{n-2}(x) \;\ldots\ldots\ldots\ldots & f_2(x) & f_1(x) & f(x) \\[6pt]
\dfrac{A_{n-1}}{n\,A_n} \;\ldots\ldots\ldots\ldots & \dfrac{A_3}{4\,A_4} & \dfrac{A_2}{3\,A_3} & \dfrac{A_1}{2\,A_2} \\[10pt]
\dfrac{2\,A_{n-2}}{(n-1)\,A_{n-1}} \;\ldots\ldots\ldots & \dfrac{2\,A_2}{3\,A_3} & \dfrac{A_1}{A_2} & \dfrac{2\,A_0}{A_1}.
\end{array}$$

and further, that when there is a discrepancy between the leading figures furnished by the two expressions used, the roots, if real, are about to separate. Now, without applying to any external source, or extending the development beyond

the stage thus reached, the character of the discrepancy adverted to will, of itself, decide the doubtful point; for the disagreement may consist either in the first expression giving a greater figure than the second, or the second a greater figure than the first: if the former happen, the roots sought will be *real*; if the latter, they will be *imaginary*. For it is plain that in the quadratic, to which the approximation tends (Equations, p. 262), we shall have, for $f(x), n=2$; for $f_1(x)$, $n=3$; for $f_2(x)$, $n=4$, and so on; so that the conditions previously given supply, in these cases, the following criteria of the character of the roots sought, that is the roots are real or not, according as these conditions exist or fail:—

$$\frac{A_1}{2\,A_2} > \frac{2\,A_0}{A_1}$$

$$\frac{A_2}{3\,A_3} > \frac{A_1}{A_2}$$

$$\frac{A_3}{4\,A_4} > \frac{2\,A_2}{3\,A_3}$$

$$\vdots$$

$$\frac{A_{n-1}}{n\,A_n} > \frac{2\,A_{n-2}}{(n-1)\,A_{n-1}}.$$

The value of these criteria, in connexion with the rapid mode of approximation taught in the work referred to, is obvious. In pursuing a pair of contiguous roots of $f_m(x)=0$, conformably to that method, we are to seek the development of the intervening root of $f_{m+1}(x)=0$, the successive figures of which, after a step or two, are always furnished by the concurring expressions above, and are to carry on the work up to $f(x)$; continuing the process as long as those expressions agree in giving the same leading figure. When this agreement ceases, the roots may be pronounced *real*, if the first expression *exceed* the second; otherwise they will be *imaginary*. And thus their character unfolds itself spontaneously, without any appeal to external tests or supplementary transformations.

The ultimate quadratic thus attained may with propriety be called the *indicator* of the doubtful roots; when it proves their reality, we may employ it to supply the leading figures of the two roots, which become distinct after the indicator is reached. This same indicator will also furnish an approximation even to the remaining imaginary portion of the par-

tially developed roots, when an imaginary pair is indicated, provided $f_m(x)$ has approximated closely to zero.

Moreover, when three roots concur in several leading figures, we should in like manner arrive at a cubic indicator; and when there are four such roots, at an indicator of the fourth degree; and so on. And these indicators, like as in the quadratic, would point out the initial directions which the separated roots take. But it is unnecessary to examine these indicators of the higher orders, all of which are ultimately reducible to quadratics: so that in examining minute intervals, in the theory of equations, like as in discussing the elements of a curve surface, the quadratic indicator is sufficient to supply, in both cases, all the desired information.

I shall now return to the general criteria at first given, and shall show their importance in detecting imaginary roots, previously to any actual development, and solely from an examination of the proposed coefficients; and shall thence deduce the rules of Newton before adverted to.

It is well known that for the purpose of examining into the character of the roots of an equation, as to real and imaginary, we may replace that equation by a series of limiting equations of inferior degree: as for instance, if the equation be above the third degree, by a series of cubic equations; or, if we please, by a series of quadratics. In the present inquiry it will be proper to take the limiting cubics, and not the quadratics, as Maclaurin, and all other investigators of Newton's rule have, I believe, done.

If any of these limiting cubics indicate imaginary roots, such indications will of course also imply imaginary roots in the proposed equation. But several indications, apparently distinct, may offer themselves in these equations, which upon closer examination may be found to be necessarily dependent, and thus to concur in pointing to but a single imaginary pair. Distinct imaginary pairs can be inferred only from distinct independent conditions: we have therefore to inquire how these are to be discovered in the series of limiting equations alluded to.

1. And first we may remark that a cubic equation consists of only four terms; and as but one imaginary pair can enter into it, it follows that whether the criterion at page 186 hold with respect to the three leading terms, or with respect to the three final terms, or in reference to both sets of three, one imaginary pair, and one only, is implied.

2. The cubics we are considering are so connected together, that if the criterion hold, or fail, in reference to the three *leading* terms of one, it must of necessity, in like manner, hold,

or fail, in reference to the three *final* terms of that next in order.

Hence the condition holding for the three leading terms of one cubic necessitates its holding for the three final terms of the next, so that the concurrence implies but a single imaginary pair. By examining our series of cubics, with these principles before us, applying the test to each group of three terms in succession, we shall obviously be able to distinguish those conditions which are really distinct and independent, from those that are not, and therefore to infer so many distinct imaginary pairs.

If the first set of three, that is the leading terms in the first cubic, satisfy the criterion, we immediately infer the existence of one imaginary pair; if the next set—the final terms of the same cubic—also satisfy it, the preceding condition merely recurs, and supplies no new information. In this case the following set of three—the leading terms of the next cubic—must furnish the same concurring condition, by the second principle above; and so on, till we arrive at a set of three for which the criterion fails, thus putting a stop to the series of concurring conditions, and preparing the way for new and independent conditions. As soon as the criterion again holds, the condition, being altogether independent of the former, must imply another and distinct imaginary pair; and so on, to the end of the series.

Now the criteria which we have here supposed to be applied to the terms, taken three at a time, of the successive limiting cubics, supply one after another the very expressions that we have exhibited at page 187; the three final terms of one cubic always furnishing the same expressions as the three leading terms of the next, as noticed above. Consequently, without the formal intervention of the limiting cubics, which have merely been introduced into the reasoning for the purpose of tracing the dependent conditions, we may at once apply the criteria (page 187) to the coefficients of the proposed equation, observing that when the condition recurs, in proceeding from one set of three to the next, the recurrence is to be regarded merely as a repetition of the same indication; that as soon as it fails, preparation is made for the occurrence of a new indication, and so on.

Hence the indications that are really independent, and consequently the number of imaginary roots inferrible from them, may be thus noted. Under the first and last terms of the proposed equation write the sign *plus*; then, taking each of the intermediate terms in succession for a middle term, write under it the sign *minus* when the criterion holds, and *plus*

when it fails: the alternations of sign, thus furnished, will denote the number of imaginary roots, at least, which enter the equation: and this is Newton's rule.

The rule now established will be found a valuable adjunct in the modern theory of numerical equations; more especially in connexion with the researches of Fourier. We shall apply it to an example taken from the *Analyse des Equations* of this author:—

$$x^5 + x^4 + x^3 - 2x^2 + 2x - 1 = 0$$
$$+ \quad - \quad + \quad - \quad - \quad +$$

Hence the equation has four imaginary roots. In the work from which this example is taken, a good deal of labour is expended upon arriving at this conclusion.

To what has now been done it may be proper to add, that although the criteria at page 187 have been deduced from Sturm's theorem, yet they may be readily inferred from independent principles; and, moreover, without any direct reference to the limiting equations of Newton and Maclaurin, adverted to above. For it is shown in the Theory of Equations, p. 323, that if the general equation

$$A_n x^n + A_{n-1} x^{n-1} + A_{n-2} x^{n-2} + \ldots\ldots\ldots\ldots A_0 = 0$$

be transformed into another, by substituting $x+r$ for x, then the third coefficient of the transformed equation, in order that the second may vanish, must be

$$\frac{A_{n-2}}{A_n} - \frac{n(n-1)}{2} \left\{ \frac{A_{n-1}}{n A_n} \right\}^2$$

Consequently if, when this evanescence takes place, the expression here written have the same sign as A_n, the zero, then occurring between like signs, will indicate imaginary roots. Hence, multiplying by the positive quantity $2 n A_n^2$, two imaginary roots will be indicated provided we have the condition

$$2n A_{n-2} A_n > (n-1) A^2_{n-1}$$

or, by reversing the coefficients, provided we have the condition

$$2n A_0 A_2 > (n-1) A_1^2$$

and this, applied to the final terms of the successive derived equations, will obviously furnish the series of criteria at p. 187. As imaginary roots are equally indicated though the third coefficient actually vanish along with the second—except all the roots are equal to r—it follows, as at p. 186, that the sign of inequality may be changed into that of equality without disturbing the indications.

In terminating these investigations, I may perhaps be per-

mitted to remark, that several interesting inquiries of a kindred nature are suggested by them: the prosecution of these I propose to publish in a distinct form, as a supplement to the volume already referred to.

J. R. Young.

XLIII. *On* M. Jacobi's *Theory of Elliptic Functions. By the Rev.* Brice Bronwin*.

In page 36 of his *Fundamenta Nova*, &c., M. Jacobi, making $\omega = \frac{m\,\mathrm{K} + m'\,\mathrm{K}'\sqrt{-1}}{n}$, says that m and m' may be any integer numbers, positive or negative, provided they have no common factor, which also measures n. What I intend in this paper is, to prove that m must be an odd and m' an even number, and that no other form is admissible. If r and r' be integers, positive or negative, the value of ω, as above defined, includes the four following forms:—

$$\left.\begin{aligned}
\omega &= \frac{(2r+1)\,\mathrm{K} + 2r'\,\mathrm{K}'\sqrt{-1}}{n}, \sin a\,m\,n\,\omega = \pm 1, \cos a\,m\,n\,\omega = 0.\\
\omega &= \frac{2r\,\mathrm{K} + 2r'\,\mathrm{K}\sqrt{-1}}{n}, \sin a\,m\,n\,\omega = 0, \cos a\,m\,n\,\omega = \pm 1.\\
\omega &= \frac{2r\,\mathrm{K} + (2r'+1)\,\mathrm{K}'\sqrt{-1}}{n}, \sin a\,m\,n\,\omega = \pm\infty\sqrt{-1}, \cos a\,m\,n\,\omega = \pm\infty.\\
\omega &= \frac{(2r+1)\,\mathrm{K} + (2r'+1)\,\mathrm{K}'\sqrt{-1}}{n}, \sin a\,m\,n\,\omega = \pm\frac{1}{k}, \cos a\,m\,n\,\omega = \pm\frac{k'}{k}\sqrt{-1}.
\end{aligned}\right\} \quad (\mathrm{A.})$$

The values of $\sin a\,m\,n\,\omega$, $\cos a\,m\,n\,\omega$ are annexed on account of their importance in what follows. They are calculated by the formulæ at pages 32 and 34. The references here made are all to the *Fundamenta Nova*, and the notation adopted there is employed here, unless express mention be made to the contrary. But to abridge I shall write $\mathrm{s}\,a\,u$ for $\sin a\,m\,u$, $\mathrm{c}\,a\,u$ for $\cos a\,m\,u$, &c.

At page 38 we have for the general transformation,

$$\mathrm{s}\,a\,\frac{u}{\mathrm{M}} = \mathrm{C}\,\mathrm{s}\,a\,u\,\mathrm{s}\,a(u+4\omega)\,\mathrm{s}\,a(u+8\omega)\ldots\mathrm{s}\,a(u+4(n-1)\omega), \quad (1.)$$

or

$$\mathrm{s}\,a\,\frac{u}{\mathrm{M}} = \frac{\frac{x}{\mathrm{M}}\left(1-\frac{x^2}{\mathrm{s}^2 a\,4\,\omega}\right)\left(1-\frac{x^2}{\mathrm{s}^2 a\,8\,\omega}\right)\cdots\left(1-\frac{x^2}{\mathrm{s}^2 a\,2\,(n-1)\,\omega}\right)}{(1-k^2x^2\,\mathrm{s}^2 a\,4\,\omega)(1-k^2x^2\,\mathrm{s}^2 a\,8\,\omega)\ldots(1-k^2x^2\,\mathrm{s}^2 a\,2\,(n-1)\,\omega)} \quad (2.)$$

Here $\frac{1}{\mathrm{C}}$ is put for the constant denominator of the second member of (1.) The middle factor of that member is

* Communicated by the Author.

$\mathrm{s}a\,(u + 2\,(n - 1)\,\omega)$, and the following factors easily reduce to $\pm\,\mathrm{s}\,a\,(u + 2\,\omega)$, $\pm\,\mathrm{s}\,a\,(u + 6\,\omega)$, &c., whatever form ω takes. Also in (2.) $\mathrm{s}\,a\,(2\,n - 2)$, $\mathrm{s}\,a\,(2\,n - 6\,\omega)$, &c. reduce to $\pm\,\mathrm{s}\,a\,2\,\omega$, $\pm\,\mathrm{s}\,a\,6\,\omega$, &c. Therefore (1.) and (2.) reduce to

$$\mathrm{s}\,a\,\frac{u}{\mathrm{M}} = \mathrm{C}\,\mathrm{s}a u\,\mathrm{s}a\,(u + 2\,\omega)\,\mathrm{s}a\,(u + 4\,\omega) \ldots \mathrm{s}a\,(u + 2\,(n - 1)\,\omega). \quad (3.)$$

$$\mathrm{s}\,a\,\frac{u}{\mathrm{M}} = \frac{\frac{x}{\mathrm{M}}\left(1 - \frac{x^2}{\mathrm{s}^2\,a\,2\,\omega}\right)\left(1 - \frac{x^2}{\mathrm{s}^2\,a\,4\,\omega}\right)\ldots\left(1 - \frac{x^2}{\mathrm{s}^2\,a\,(n - 1)\,\omega}\right)}{(1 - k^2 x^2\,\mathrm{s}^2\,a\,2\,\omega)\,(1 - k^2 x^2\,\mathrm{s}^2\,a\,4\,\omega) \ldots (1 - k^2 x^2\,\mathrm{s}^2\,a\,(n - 1)\,\omega} . \quad (4.)$$

The quantity C, if we give it M. Jacobi's form, will reduce in like manner; but this is of no consequence. M. Jacobi appears to have been aware of the reduction above effected, as he has partially made it at pages 41 and 51.

It hence appears that the second form in (A.) will always reduce to one of the three other forms. For making $\omega' = \frac{r\,\mathrm{K} + r'\,\mathrm{K}'\,\sqrt{-1}}{n}$, we have $\omega = 2\,\omega'$; and putting $2\,\omega'$ for ω in (3.) and (4.), these last become of the form (1.) and (2.); which will again reduce to the forms (3.) and (4.), ω' being in the place of ω. And if ω' be divisible by 2, we may repeat the operation, and may continue to do so till we arrive at an $\omega'''' = \frac{p\,\mathrm{K} + q\,\mathrm{K}'\,\sqrt{-1}}{n}$, in which one or both the quantities p and q are odd numbers. The same reduction might also be made in the values of $\mathrm{c}\,a\,\frac{u}{\mathrm{M}}$ and $\Delta\,a\,\frac{u}{\mathrm{M}}$. When, therefore, ω is divisible by 2, the formulæ of transformation will reduce till ω takes one of the three other forms.

Again, the second members of (1.) and (2.) are not proper representations of the first. For since $\mathrm{s}a\,2\,(n - 1)\,\omega = \pm\,\mathrm{s}a\,2\,\omega$, $\mathrm{s}\,a\,2\,(n - 3)\,\omega = \pm\,\mathrm{s}\,a\,6\,\omega$, &c.; these members vanish when $u = 2\,\omega$, $u = 6\,\omega$, &c., and therefore ought to contain the factors $1 - \frac{x^2}{\mathrm{s}^2\,a\,2\,\omega}$, $1 - \frac{x^2}{\mathrm{s}^2\,a\,6\,\omega}$, &c. It must be remembered that $x = \mathrm{s}\,a\,u$. For the same reason the second members of (3.) and (4.) would be improper representations of the first, if ω were divisible by 2. The second form of (A.) therefore is inadmissible.

For the three other forms of ω, we cannot reduce $\mathrm{s}a\,(n - 1)\,\omega$ to $\pm\,\mathrm{s}\,a\,\omega$, $\mathrm{s}\,a\,(n - 3)\,\omega$ to $\pm\,\mathrm{s}\,a\,3\,\omega$, &c.; and it is not possible to reduce them to sines of any other amplitudes. For these values of ω, then, (3.) and (4.) are in their simplest forms; their second members vanish when $u = 0, 2\,\omega, 4\,\omega$, &c., but never

between these values. Consequently, while u increases of $2\,\omega$, $\frac{u}{\mathrm{M}}$ increases of $2\,\mathrm{H}$, neither more nor less, if $\frac{u}{\mathrm{M}} = \mathrm{H}$ when its amplitude is $\frac{\pi}{2}$. Whilst, therefore, u from 0 becomes ω, $\frac{u}{\mathrm{M}}$ from 0 becomes H. Let them have these values in (3.), and we obtain $\mathrm{s}\,a\,\mathrm{H} = 1 = \pm\,\mathrm{C}\,\mathrm{s}\,a\,\omega\,\mathrm{s}\,a\,3\,\omega \ldots\ldots \mathrm{s}\,a\,(2\,n-1)\,\omega$. Therefore

$$\frac{1}{\mathrm{C}} = \pm\,\mathrm{s}\,a\,\omega\,\mathrm{s}\,a\,3\,\omega\,\mathrm{s}\,a\,5\,\omega \ldots\ldots \mathrm{s}\,a\,(2\,n-1)\,\omega. \quad . \quad (5.)$$

This then is the general form of $\frac{1}{\mathrm{C}}$, and M. Jacobi's denominator cannot be true, except in those cases in which it is reducible to it.

One factor of (5.) is $\mathrm{s}\,a\,n\,\omega$. The second and third forms (A.), therefore render C faulty, and also the values of $\mathrm{s}\,a\,\frac{u}{\mathrm{M}}$, of M, and of the modulus of $\frac{u}{\mathrm{M}}$ faulty; for C enters into these values. If M. Jacobi's formulæ do not fail for these values of ω, it is because they do not hold true for them. His value of $\frac{1}{\mathrm{C}}$ is

$$\{\mathrm{s}\,a\,(\mathrm{K}-4\,\omega)\,\mathrm{s}\,a\,(\mathrm{K}-8\,\omega) \ldots\ldots \mathrm{s}\,a\,(\mathrm{K}-2(n-1)\,\omega)\}^2$$

$$= \left\{\frac{\mathrm{c}\,a\,4\,\omega\,\mathrm{c}\,a\,8\,\omega \ldots\ldots \mathrm{c}\,a\,2\,(n-1)\,\omega}{\Delta.a\,4\,\omega\,\Delta.a\,8\,\omega \ldots\ldots \Delta.a\,2\,(n-1)\,\omega}\right\}^2$$

$$= \left\{\frac{\mathrm{c}\,a\,2\,\omega\,\mathrm{c}\,a\,4\,\omega \ldots\ldots \mathrm{c}\,a\,(n-1)\,\omega}{\Delta.a\,2\,\omega\,\Delta.a\,4\,\omega \ldots\ldots \Delta.a\,(n-1)\,\omega}\right\}^2,$$

whatever be the form of ω. But for the first of the forms (A.) only can we have

$$\mathrm{s}\,a\,\omega\,\mathrm{s}\,a\,3\,\omega \ldots\ldots \mathrm{s}\,a\,(2n-1)\,\omega = \{\mathrm{s}\,a\,\omega\,\mathrm{s}\,a\,3\,\omega \ldots\ldots \mathrm{s}\,a\,(n-2)\,\omega\}^2$$

$$= \left\{\frac{\mathrm{c}\,a\,2\,\omega\,\mathrm{c}\,a\,4\,\omega \ldots\ldots \mathrm{c}\,a\,(n-1)\,\omega}{\Delta.a\,2\,\omega\,\Delta.a\,4\,\omega \ldots\ldots \Delta.a\,(n-1)\,\omega}\right\}^2.$$

For the other forms of ω this reduction cannot be effected. M. Jacobi's formulæ therefore are only true for the first form (A.) In page 39 we have

$$\mathrm{c}a\frac{u}{\mathrm{M}} = \frac{\mathrm{c}a\,u\,\mathrm{c}a\,(u \div 4\,\omega)\,\mathrm{c}a\,(u+8\,\omega) \ldots \mathrm{c}a\,(u+4\,(n-1)\,\omega)}{\{\mathrm{c}a\,4\,\omega\,\mathrm{c}a\,8\,\omega \ldots \mathrm{c}a\,2(n-1)\,\omega\}^2} \,. \quad (6.)$$

This reduces as before to

$$\mathrm{c}a\frac{u}{\mathrm{M}} = \pm\,\frac{\mathrm{c}a\,u\,\mathrm{c}a(u+2\,\omega)\,\mathrm{c}a\,(u+4\,\omega) \ldots \mathrm{c}a\,(u+2\,(n-1)\,\omega)}{\{\mathrm{c}a\,2\,\omega\,\mathrm{c}a\,4\,\omega \ldots \mathrm{c}a\,(n-1)\,\omega\}^2}. \quad (7.)$$

This last must vanish when $u = \omega$, $3\,\omega$, $5\,\omega$, &c., because then $\frac{u}{\mathrm{M}} = \mathrm{H}$, $3\,\mathrm{H}$, &c. The only factor in the numerator of the second member which can vanish is $\mathrm{c}\,a\,n\,\omega$; but this vanishes only for the first of the forms (A.), which therefore is the only admissible form. Moreover, the second of (A.) makes $\mathrm{s}\,a\,\mathrm{H}$, $\mathrm{s}\,a\,3\,\mathrm{H}$, &c. $= 0$, which is absurd. The third of (A.) gives a factor infinite in both $\mathrm{s}\,a\,\mathrm{H}$, $\mathrm{c}\,a\,\mathrm{H}$, &c.

If we develope (7.), it should give

$$\mathrm{c}a\frac{u}{\mathrm{M}} = \sqrt{1-x^2}\,\frac{\left(1-\frac{x^2}{\mathrm{s}^2 a\,\omega}\right)\left(1-\frac{x^2}{\mathrm{s}^2 a\,3\,\omega}\right)\ldots\left(1-\frac{x^2}{\mathrm{s}^2 a\,(n-2)\,\omega}\right)}{(1-k^2x^2\mathrm{s}^2 a\,2\,\omega)(1-k^2x^2\mathrm{s}^2 a\,4\,\omega)\ldots(1-k^2x^2\mathrm{s}^2 a\,(n-1)\,\omega)} \quad (8)$$

The first formula in page 39, which is the development of (6.), should reduce to (8.), which vanishes for all the odd multiples of ω. But these reductions cannot be effected, except for the first of the forms (A.). If we deduce (8.) immediately from (4.), as Sir James Ivory has done, we find that $\sqrt{1-x^2}$ results from $\sqrt{1-\frac{\mathrm{s}^2\,a\,u}{\mathrm{s}^2\,a\,n\,\omega}}$, which indeed is evident. This excludes all the values of ω but the first, for this factor must vanish when $x = \mathrm{s}\,a\,u = \pm\,1$. And hence it is evident that the form of M. Jacobi's theory excludes all other values of ω but the first.

From what has been done, it is abundantly plain that M. Jacobi's transformation is true only for the form

$$\omega = \frac{(2\,r+1)\,\mathrm{K} + 2\,r'\,\mathrm{K}'\,\sqrt{-1}}{n};$$

and, moreover, that if we substitute for $\frac{1}{\mathrm{C}}$ the general value (5.) instead of his value, it would fail for all the other forms of ω. The only possible value of ω, therefore, is the preceding; consequently many of the forms suggested at pages 49 and 50 would fail, as $\frac{i\,\mathrm{K}'}{n}$, $\frac{\mathrm{K}+i\,\mathrm{K}'}{n}$, $\frac{\mathrm{K}+3\,i\,\mathrm{K}'}{n}$, $\frac{2\,\mathrm{K}+i\,\mathrm{K}'}{n}$, $\frac{3\,\mathrm{K}+i\,\mathrm{K}'}{n}$, $\frac{(n-1)\,\mathrm{K}+i\,\mathrm{K}'}{n}$, and some others. The particular transformation given in pages 52, 53, 54, 55 must fail; and as we cannot make $\omega = \frac{m\,\mathrm{K}'\,\sqrt{-1}}{n}$, we cannot obtain a real transformation by means of imaginary quantities. Indeed it does not appear that any other form than $\omega = \frac{(2\,r+1)\,\mathrm{K}}{n}$ would be of any utility; and this last, it is easy to see, will

reduce to $\omega = \frac{K}{n}$; for the functions of all amplitudes greater than $u + 2(n-1)\omega$ would reduce to similar functions of amplitudes less than this.

In the 15th Number of the Cambridge Mathematical Journal, I very briefly pointed out the failure of some of M. Jacobi's forms for ω. This the editor of that work was very reluctant to admit; and in the 16th Number of the same I find a Note, bearing the signature C., intended as a refutation of what I had advanced. On this Note I must now make a few observations, but after what has been done in this paper it will not be necessary to enter far into particulars.

If the writer wished to compare my denominator with M. Jacobi's, he should, for each particular form of ω, have attempted to reduce them to the same quantity. His method gives the true one for the first value of ω, which proves to be mine, and affords no foundation for the inferences he has drawn from it. The true form would also be ascertained the moment it is proved, that when $u = \omega$, $v = \text{H}$. And this is the important point; if they differ, and when they differ, which is right. For the second value of ω, the formulæ (1.), (2.) admit of reduction, as already shown. [The references are made to the note.] The third gives a factor infinite in the numerators of the second members of (1.), (2.) when $u = \omega$, and is therefore evidently inadmissible. For the last value of ω, $\text{s}\,a\,n\,\omega = \pm\frac{1}{k}$, $\text{c}\,a\,n\,\omega = \pm\frac{k'}{k}\sqrt{-1}$; and these values the writer of the note has, evidently by mistake, taken for the values of $\text{s}\,a\,v$, $\text{c}\,a\,v$. For surely he never could prove, that when $u = \omega$ the second member of (2.), divided by $\text{c}\,a\,n\,\omega$, is equal to unity. His last value of C therefore is wrong. I feel compelled to say that this Note is perfectly absurd at every step of it; and if the author had proved what he aimed at, and which is really true, it would have been nothing to the purpose.

Denby, near Huddersfield, Jan. 5th, 1843.

XLIV. *On* Mr. Earnshaw's *Deduction of a Property of Circularly Polarized Light. By Professor* Powell.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In a late Number of your Journal there appears a *theoretical* deduction by Mr. Earnshaw to this effect, that if circularly polarized light, *right*-handed, for example, fall on glass at a

perpendicular incidence, the reflected ray will be *left*-handed, and *vice versâ*.

I have been prevented for some time from attending to the subject, but having now done so, and not observing that any of your other correspondents have taken up the subject, I beg to announce that I have succeeded in *verifying* by experiment the above theoretical prediction.

My experiment was conducted by the use of Mr. Airy's "new analyser" described in the Cambridge Transactions, 1832, which does the same for *right*- and *left*-handed circular light, as the common analyser does for light polarized in opposite planes; that is, it stops one kind and transmits the other. This affords a ready test whether any given ray is right- or left-handed. I procured the circular light by means of the Fresnel-rhomb, and on ascertaining that the light emerging from it was *stopped* by the Airy-analyser, I examined the same light after reflexion from glass at an incidence as near the perpendicular as possible, and found it *transmitted* by the analyser; thus proving its change from right- to left-handed.

I remain, Gentlemen,

Your most obedient Servant,

Oxford, March 5, 1843. B. POWELL.

XLV. *On the Form of Crystals of Tin.* By W. H. MILLER, *M.A., F.R.S., Professor of Mineralogy in the University of Cambridge*.*

ALTHOUGH crystals of tin have been not unfrequently observed when the metal has been permitted to cool slowly after fusion, and also when it has been reduced by galvanic action, they appear to have been too imperfect to admit of the determination of their forms by measurement with the reflective goniometer. If however a feeble galvanic current, produced by one of Daniell's constant cells weakly charged, be transmitted through a solution of tin in hydrochloric acid, kept nearly saturated by suspending in the solution a piece of metallic tin connected with the copper element of the cell, in the course of four or five days very perfect crystals may be obtained.

These crystals belong to the pyramidal system.

The symbols of the different simple forms which have been observed, in the notation adopted in my treatise on Crystallography, will be as follows:—the letter denoting one of the faces of each form being prefixed to the symbol of the form,

$$a\{100\},\quad m\{110\},\quad p\{111\},\quad s\{101\},\quad r\{301\},\quad t\{331\}.$$

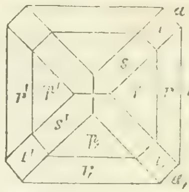
* Communicated by the Author.

It appears from a mean of the best of upwards of five hundred observed angles, that

$$\frac{1}{a} = \frac{0\cdot3857}{c},$$

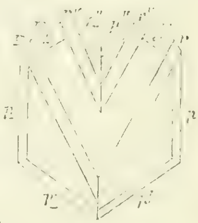
and that the angles between normals to the different faces are

$a\,m$	45° 0′	$m\,t$	57° 39′·3
$m\,a_{\prime}$	45 0	$a\,s$	68 54·5
$p\,p'$	57 13	$a\,t$	40 50
$s\,s'$	42 11	$m\,p$	61 23·5
$t\,t'$	98 20	$m\,r$	31 26
$r\,r'$	117 8	$p\,p_{\prime}$	39 35
$a\,p$	70 12·5	$r\,r_{\prime}$	74 13·2
$a\,r$	52 53·4	$s\,s_{\prime}$	29 29
$m\,s$	75 15·5	$t\,t_{\prime}$	64 41·3



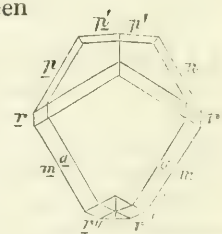
Twin crystals occur very frequently, the twin axis being either perpendicular to p or to r.

In the crystals having the twin axis perpendicular to p, the angles between normals to the faces are



$\underline{p}'\,p'$	65° 34′	$\underline{m}\,m$	57° 13′
$\underline{r}\,r$	120 5	$\underline{r}''\,r''$	− 5 39

In the crystals having the twin axis perpendicular to the face r, the angles between normals to the faces are



$\underline{p}\,p'$	5° 39′	$\underline{m}\,m$	117 8′
$\underline{p}p$	120 5	$\underline{r}''\,r''$	54 16

Some very slender capillary crystals of tin, said to have been obtained by fusion for which I am indebted to Mr. Brooke, are regular eight-sided prisms, apparently a combination of the forms to which the faces a, m belong. The crystalline markings seen on the surface of tin after cooling down from a state of fusion very closely resemble the confused crystallization which is occasionally produced when the metal is reduced by the galvanic current. Hence in all probability the crystals obtained by fusion have the same form as those produced by galvanic action.

At 10°·5 C. the specific gravity of the crystals divided by that of water is 7·178. At the same temperature the specific gravity of a mass obtained by fusing the crystals divided by that of water was found to be 7·293.

In conclusion I may observe, that tin is the only simple substance yet known, the crystals of which belong to the pyramidal system, and the only crystallizable metal not belonging either to the octahedral or rhombohedral system. It is also I believe the first instance in which the action of voltaic electricity has led to an accurate knowledge of a new crystalline species.

Cambridge, Feb. 3, 1843. W. H. Miller.

P.S. Since the above was sent for insertion in the Philosophical Magazine, I met with the following passage in a memoir by Professor Frankenheim, entitled "*System der Krystalle*," forming part of vol. xix. of the *Nova Acta Acad. Nat. Cur.* "According to Breithaupt, tin occurs in hexagonal prisms in the tin furnaces of Cornwall. By reduction at a low temperature I have always obtained it in tesseral forms." The hexagonal prisms of Breithaupt are in all probability an alloy of tin and copper Cu Sn^2, crystals of which from a specimen in the Mineralogical Museum of Strasburg, were described by me in the Philosophical Magazine for February 1835. It does not appear whether Frankenheim observed any forms of the octahedral system excepting three faces at right angles to each other. The occurrence however of three faces at right angles to each other, though not an absolute proof that the crystals belong to the octahedral and not to the pyramidal system, would afford a strong presumption that they did in the present instance, inasmuch as I have never been able to detect the slightest indication of a face perpendicular to the axis of the pyramid.

Cambridge, March 8, 1843. W. H. M.

XLVI. *On* Sir G. C. Haughton's *Experiments in Electricity related in the last Number. By* J. P. Joule, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

ALLOW me to occupy a small portion of your space with a few observations on the subject of an interesting communication by Sir G. C. Haughton, inserted in your last Number.

On repeating the experiments on the action of frictional electricity upon the galvanometer, I find that the phænomenon to which Sir Graves has called attention is simply an example of the repulsion of bodies which are in the same electrical state, and is not sensibly owing to the minute currents which

are put into play by the electrifying machine. This may be easily shown by the following experiment.

Take half an inch of copper wire, and suspend it, by a filament of silk, in the centre of a vertical insulated metallic ring having an aperture of about an inch and a quarter. Electrify positively both the ring and the copper wire, and the latter will immediately place itself at right angles to the plane of the former. Now cause the needle to communicate with the ground for an instant; and, becoming of course negatively electrical by induction, it will be immediately *attracted* to the plane of the ring, and will vibrate from one side to another of it with the same energy as it did about the angle of 90°, when positively electrical.

It is manifest, that the unstable equilibrium of the wire, when it is similarly electrical with the ring, and is also exactly in the same plane, may be converted into a stable equilibrium by superadding an attractive force which urges it to remain in that meridian. Hence it is that Sir G. C. Haughton found that a delicately suspended magnetic needle remained steady, though a needle without directive energy was in similar circumstances deflected to 90°.

The interesting and important case of static electrical action observed by Sir G. C. Haughton ought to be carefully guarded against by those who wish to ascertain the quantity of current frictional electricity by the multiplier. In France this instrument has been employed by M. Peltier for measuring the electricity of the atmosphere; and whilst it cannot be doubted that that able electrician used great precautions, the danger of the apparatus receiving a charge of electricity sufficient to interfere seriously with the results, cannot be too strongly urged upon the attention of those who repeat his experiments.

I remain, Gentlemen,

Yours respectfully,

James P. Joule.

In my former paper, p. 205, the second sentence should read thus:—

"According to this the corrected theoretical result is, 10°·47 minus one quarter of the heat evolved by the union of water and sulphuric acid, equals about 9°·47."

Broom Hill, Pendlebury,
near Manchester, March 4, 1843.

XLVII. *On the Immersion of a small Spherical Ball in a Jet of high-pressure Steam.* By A CORRESPONDENT.

IN a paper which appeared in the last (January) Number of this Magazine, Mr. Armstrong makes us acquainted with the curious circumstance, that when a small spherical ball is hung by a thread in a jet of high-pressure steam a sensible force is required to draw it out: I shall endeavour shortly to account for this phænomenon.

The most obvious explanation which presents itself is, that the lateral pressure of the steam is greater nearer the side of the jet than it is internally, but as the pressure at the surface can only be equal to the atmospheric pressure, this method of explanation seems to be attended with some difficulty; nevertheless there can be little doubt it is the true one.

Suppose we have a vertical tube inserted in the steam-boiler having a piston capable of moving within it. When the piston is allowed to yield to the pressure of the steam, it will move along the tube until it either remains at rest or retrogrades.

Let E represent the pressure of the steam on the piston at any instant, e the atmospheric pressure in a state of rest; then if we consider the pressure of the air on the external surface of the piston not sensibly to vary from the atmospheric pressure, the piston will be urged along by a variable force E e. It is perfectly clear that when the piston ceases to move forwards the force Ee must have changed its sign, or the elastic force of the steam on the surface of the piston must be then less than the atmospheric pressure. The above proceeds on the supposition that the atmospheric pressure on the external surface of the piston does not sensibly vary during the motion: but in the case supposed this could not possibly be true. Nevertheless, if instead of a fixed tube with a piston moving in it we had a moveable tube with its external aperture closed and sliding in an orifice in the boiler, the extra atmospheric force called into play by the motion would be less than in the former case; and this nearly approximates to what must happen in the central part of the jet of steam when it is allowed to escape with perfect freedom from the boiler. Now it is impossible to estimate this extra force of resistance called into play by the motion, which will of course depend upon the velocity, and that according to a law of which we have no knowledge. Yet it is perfectly conceivable that the law of resistance may be such, as that in the first instance the actual force of resistance should be less than the elastic force of the steam in the jet. The consequence of this would be, that the

velocity of the piston (to recur to our old example) would in the first instance go on increasing, the resistance also increasing with it, at the same time that elastic force of the steam upon the piston is constantly diminishing; so that the two antagonist forces must gradually become equal.

The piston, however, would still for some time continue to move forward by reason of the velocity it has acquired, so that ultimately the elastic force of the steam on the piston would be less than the resistance of the atmosphere. Now it is obvious, if the jet be not large, that the resistance of the atmosphere, when perfectly free to expand in every direction, would not be very materially greater than the elastic force of the same fluid at rest; and we may thus see how possible it is that at a certain distance from the orifice the elastic force of the steam in the centre of the jet (and by consequence the lateral pressure) should be less than the pressure at the surface, which, as we remarked before, is equal to the atmospheric pressure.

The same mode of explanation is applicable to another curious and well-known phænomenon, I mean where a circular disc is fixed at the end of a tube by which it is pierced centrically.

In this case, as is well known, if a paper disc of the same size, or smaller than that which is fixed to the end of the tube, be brought near to the latter, it may be made to adhere to it by simply blowing in at the other end of the tube. The obvious explanation is, that the pressure of the air between the two discs (or at least of a certain portion of it) is less during the motion than the pressure of the atmosphere in a state of rest.

London, Jan. 28, 1843. R. M.

XLVIII.—*On* Dr. Hare's *Second Letter, and on the Chemical and Contact Theories of the Voltaic Battery. By* Michael Faraday, *Esq., D.C.L., F.R.S.*

To R. Taylor, Esq.

My dear Sir,

YOU are aware that considerations regarding health have prevented me from working or reading in science for the last two years. This will account to you for my ignorance of the circumstance that you had reprinted Dr. Hare's second letter to me*; and I believe I knew it only for the first time a week or two ago, on beginning to read up. As some persons think a letter unanswered is also unanswerable, I

* Phil. Mag. 1841, vol. xviii. p. 465.

write merely to say, that when it was sent to me as printed in Silliman's Journal, I sent a brief letter back, declining to enter into discussion, since I had nothing more to say than had been said, and still thought that that was sufficient to enable my own mind to rest in the view it had taken of static induction, &c. My reason for declining was no want of respect to Dr. Hare, but a strong conviction that controversial reply and rejoinder is but a vain occupation. Professor Silliman wrote me word that he had very unfortunately lost my brief note, but hoped to find it and print it. Since then I have forgotten the matter, and only renew it to give the same sort of answer to the letter as contained in your Journal.

I perceive also in your Magazine several attacks, from Germany, Italy and Belgium, upon the chemical theory of the voltaic battery, and some of them upon experiments of mine. For my own part I refrain from publicly noticing these arguments, simply because there is nothing in them which suggests to my mind a new thought illustrative of the subject, or gives any ground for a change in my opinion. But whilst speaking on this point I cannot help expressing a wish that some of the advocates of the contact theory would touch upon the consideration which, up to this time, seems to have been most carefully avoided, namely the unphilosophical nature of the assumed contact force, as I have endeavoured to express it in par. 2065 to 2073 of my "Experimental Researches," and as Dr. Roget has expressed it in words which I have appended in a note to my paper. Such a consideration seems to me to remove the *foundation itself* of the contact theory. I wish you could be persuaded to think it worth while to reprint those three pages in your Magazine*. As far as I can perceive, they express a fundamental principle which cannot be set aside or evaded by a philosophical mind possessing only a moderate degree of strictness in its reasonings; and I must confess, that until some answer, or some show of answer in the form of assumption or otherwise, is made to that expression of what I believe to be a law of nature, I shall feel very little inclined to attach much importance to facts which, though urged in favour of the contact theory, are ever found by the partisans of the chemical theory just as favourable to, and consistent with, their peculiar views.

I am, my dear Sir,
Very faithfully yours,
M. Faraday.

Royal Institution, March 11, 1843.

* We purpose to insert these pages in our next Number.—Ed.

XLIX. *Some Experiments and Remarks on the Changes which Bodies are capable of undergoing in Darkness, and on the Agent producing these Changes.* By Robert Hunt, *Secretary to the Royal Cornwall Polytechnic Society.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE communications which have appeared in your Journal from Professor Draper of New York, I have studied with the greatest interest, seeing in him an able and industrious inquirer in that path of science to which I have myself directed some attention. In the pages of the Philosophical Magazines for November and December 1842 and March 1843, are three papers by Dr. Draper, in which he seeks to explain many of the phænomena with which the art of photography has brought us acquainted, and those still more mysterious effects to which the labours of M. Moser* have called general attention. With the first of these papers I have at present little to do, having in my essay on Thermography, which you did me the honour to publish in your Magazine for December, acknowledged the priority of Dr. Draper's observations on *Latent Light*. I would however notice, in reference to Sir John Herschel's valuable remarks on the Daguerreotype which accompanied that paper, which you printed in February, that your Magazine for April 1840 contains a communication on "The Chemical Action of the Solar Spectrum," accompanied by a drawing, from me, which will show that I have a prior and very distinct claim to the prismatic analysis of the Daguerreotype photograph, which Sir John Herschel has, with characteristic readiness, acknowledged in a note with which he has honoured me, dated February 17th. Professor Draper's paper "On a New Imponderable Substance, and on a Class of Chemical Rays analogous to the Rays of Dark Heat," and also that "On the rapid Detithonizing Power of certain Gases and Vapours," &c., claim our especial attention. The object of these communications is to prove that the property of effecting chemical change, which has been considered due to the rays of Light, ought to be attributed to a "new imponderable substance," which Dr. Draper proposes to call Tithonicity. Although many of the very remarkable effects which I have observed have frequently led me to consider seriously the propriety of separating the rays by which they were produced from Light

* See translations of M. Moser's treatises in Taylor's Scientific Memoirs, Part XI.—Some notices relating to the subject will be found among the Miscellaneous articles in our present Number.—Ed.

rays, and still more strongly convinced by the recent researches of Sir John Herschel of the existence of solar rays broadly distinguished from HEAT and LIGHT, I do not think we have as yet a sufficient amount of experimental evidence to warrant us in receiving that broad distinction as truth, which would refer *all chemical changes* effected by solar radiation to a new element. I should not venture to trouble you with any remarks on this subject, but that I fear the adoption of the ideas and the proposed nomenclature of Dr. Draper will involve a very complex inquiry in inextricable difficulty. My only desire is the discovery of the truth; and believing, as I do at present, that Dr. Draper has given a wrong interpretation to his very excellent experimental results, I hope he will do me the justice of considering that I am actuated by the purest principles in opposing his views; and I do assure you that I shall feel much pleasure in receiving from him, or any other inquirer, any correction, if I have drawn wrong conclusions from the results of *my* experiments.

There are several points in the communications referred to, to which I cannot give my assent, particularly to "the entire independence *throughout the spectrum* of the luminous rays that give to the organs of vision the impression of colour and the tithonic rays," but I am not prepared *to prove* the contrary. Again, "as to the independence of these rays and the rays of heat," I find myself in the same situation, although it appears to me that I have a stronger fact connected with the influence of the moonbeams than the proof brought forward by Dr. Draper, "that strong impressions of the moon's surface may be taken on sensitive plates." I will describe my experiments. Copper-plate engravings and woodcuts were placed on well-polished plates of copper, and exposed in photographic copying frames, well-guarded by flannel cushions and glass from the deposition of moisture, for one and two hours to bright moonlight. In each case most perfect copies of the prints were made on the copper, which were evident before the application of vapour, and became beautifully defined when the plates were breathed upon, but were not improved by the vapours of mercury or iodine. The unfavourable state of the weather alone has, for the present, put a stop to further inquiries. It is difficult to deal with this question in the present stage of our researches. I am inclined to regard the phænomena which I have above described, and the fact mentioned by Dr. Draper, of the moon impressing her own image on Daguerreotype plates, as belonging to the same class, and to be attributed, neither to the influence of "calorific rays," which cannot be detected in the moonbeams, nor to "chemical rays," although the light of the full

moon will darken several argentine preparations, but merely to a disturbance of the equilibrium of the caloric latent in the plates. It will be found by experiment that heat or cold applied to one portion of a metal plate will render that part more susceptible of condensing vapour than the other parts. I must confess, however, that I hold my judgement suspended; for there are curious circumstances which I am endeavouring to investigate, which appear to indicate the influence of an agent with which we are unacquainted. I will now come to the more important part of the Professor's communication,—"*Proof of the existence of* DARK TITHONIC *rays analogous to the rays of* DARK HEAT."

Dr. Draper and M. Moser do not appear to admit that the film of ioduret of silver on the Daguerreotype plates is decomposed by solar agency; the former attributing the change of colour to a disturbance produced by the absorption of the "tithonic rays," and the latter to a molecular change produced by light "externally blackening the iodide or other compound of silver."

My own impressions are certainly adverse to this. I formerly* expressed it as my opinion that the iodide was converted into an oxide of silver by the light. I now believe the oxidation to be a secondary action, due to the influence of the atmosphere. A great number of carefully conducted experiments have convinced me that the solar rays liberate iodine, leaving the silver in a state of very fine division loosely on the surface of the solid metal. Iodide of silver is insoluble in nitric acid or ammonia, although I find the former has the power of decomposing that salt by long-continued action; but it will be found that the darkened portions of the solarized Daguerreotype plates are rapidly dissolved by diluted nitric acid, and slowly by ammonia. If, as I have described in the paper already referred to, we receive an impression of the solar spectrum on an iodized plate, we may, by gently rubbing, remove all the darkened portion, and thus distinctly mark the spaces of maximum and minimum action, proving that some kind of decomposition has taken place. If we take the precipitated iodide of silver, we shall be enabled to trace more distinctly all that occurs. It has been remarked, that this salt exists in two states; the one sensible, and the other insensible to light†. I find that perfectly pure iodide of silver undergoes little or no change by long exposure, but the presence of the smallest portion of nitrate of silver renders

* Phil. Mag., N.S. vol. xvi. p. 275.

† Sir John Herschel, Phil. Trans., part 1. for 1840; Phil. Mag., S. 3, vol. xvi. p. 275.

it very sensitive to luminous agency. If we take this sensitive iodide and allow it to blacken, and then throw it into diluted nitric acid, all the darkened portion will be dissolved, and a pure non-sensitive iodide left behind; or if we throw it into ammonia the darkened part will disappear, leaving the yellow salt, and in a few hours the ammonia will be covered with a pellicle of Faraday's oxide of silver.

According to Dr. Draper, if a properly prepared plate is exposed to light until in a fit state to receive the mercurial vapour and then placed in a dark room for four or five hours with a blackened *metallic* plate suspended one-eighth of an inch from its surface, we find, when it is exposed to the vapour of mercury, that those parts only are attacked by it which correspond with the suspended screen. It is inferred from this, "that the tithonicity that had originally disturbed the surface of the plate equally all over has escaped away from those portions that were uncovered; but that its escape has been entirely prevented by the action of the screen, and this must be through radiation...And further, that the rays that do thus escape away are absolutely invisible to the eye." Dr. Draper then proceeds to point out the analogy between this case and one of a body cooling by radiation, concluding, "the two cases are absolutely alike. Tithonicity therefore radiates exactly after the manner of heat." When we can account for the effects produced by known causes, we are not justified in seeking for new ones. In my paper on Thermography*, I have shown that any blackened body placed above an iodized or a simply polished plate of metal,—whether that plate has been exposed to light or not is of no consequence,—will produce such a change as will dispose the covered portion to receive the vapour of mercury. If I can succeed in proving that all the phænomena described by Dr. Draper are to be produced as well on simple metals as on surfaces of the ioduret of silver or other sensitive surfaces, and equally in perfect darkness as in the brightest light, and that many of them are to be traced to chemical change, I shall certainly convince you of the impropriety of attributing them to the chemical, or, as Dr. Draper terms them, the tithonic rays.

As it was difficult, from the length of time required, to produce any decided effects upon the surfaces of polished metal plates by the solar spectrum without a good heliostat to keep the sun's image stationary for some hours, I was obliged to have recourse to different absorptive media in my endeavours to ascertain whether any of the rays of LIGHT

* Phil. Mag., S. 3. vol. xxi. p. 462.

were active in producing the effects described by Dr. Draper, Moser and myself. I however obtained the most decided evidence that no effect was produced by an exposure of two hours to the prismatic spectrum by the most refrangible rays; but it frequently happened in these trials, that much mercurial vapour was deposited about that part of the metal on which the rays of least refrangibility fell. These experiments were, however, by no means satisfactory; but I hope, with the increase of light and the required instruments, to examine the question minutely this summer.

The coloured media used by me were,—1, red; 2, yellow; 3, green; 4, blue: these insulated respectively the following rays:—

1. Red, orange, yellow, and some of the blue.
2. Orange, yellow, green, with a faint trace of the blue, and a small portion of the red.
3. Orange, yellow, green, and blue.
4. Green, blue, indigo, and violet.

In addition to these fluids, water (5), and water blackened with ink (6), were also used.

A very highly polished plate of copper with the above media in flint-glass flat bottles laid upon it, was exposed to bright sunshine for one hour. It was then placed in a dark box, and subjected to the vapour of mercury. The largest deposit of mercury was found to mark the space occupied by the red fluid (1); the next in order was that influenced by the blue (4), and then the others as follows: green (3), yellow (2), white (5), and black (6). There was less mercury deposited where the last four bottles of fluids had lain than on the uncovered portions of the plate. Precisely the same arrangement as the above was *kept in the dark* for five hours, and the plate then exposed to mercurial vapour, which attacked it in the same manner as when subjected to strong sunshine, except that no mercury was deposited on the uncovered parts of it.

Another copper plate was placed one-eighth of an inch above the same bottles of fluids in the dark, and allowed to remain in that position for five hours. On exposure to the vapour of mercury, it was found that the only impressions were made on those parts opposite the red fluid (1), the blue (4), and the green (3), the largest deposits corresponding with the red fluid; but that portion opposed to the green fluid (3) exhibiting the slightest possible traces of action.

I cannot explain any of the peculiarities which I have now described; they belong to a class of phænomena of the most mysterious kind, and the elucidation will only be effected by

the most unwearying perseverance in experimental observation, and by a mind capable of drawing the most logical deductions therefrom. I state them now as they have occurred, for the purpose of showing how perfectly independent these effects appear to be of the chemical rays of light. I may mention another very curious result in connexion with the above. Two copper-plate engravings were placed upon highly polished amalgamated plates of copper; one of them was covered with common window-glass, and the other with a deep red glass stained with the oxide of gold. They were exposed to daylight for four hours, during which there were but faint gleams of sunshine. On subjecting the plates to the vapour of mercury, a very capital copy of the print was found to have been made under the influence of the red glass, but no trace of an impression under the other. It will be seen, on referring to Dr. Draper's paper, that these results correspond mainly with some obtained by him; but they will not admit of the explanation he has given of the radiation of tithonicity, unless he can in the first place prove the constant presence of the agent he has so named in all bodies. I know he attempts to do this, but I am by no means satisfied that either Moser or Draper have as yet proved either the existence of "invisible light," or "dark tithonic rays."

With the view of testing Dr. Draper's results, I carefully iodized two silver plates and exposed them to light. I then placed them so that half of one plate was covered by half of the other, and allowed them to remain in the dark $\frac{1}{24}$th of an inch apart for four hours. On mercurialization I could not detect the slightest difference between the covered and the uncovered portions of either of the plates.

Another silver plate was iodized and exposed to light. It was then placed in the dark with a sensitive plate which had been carefully kept from the light $\frac{1}{16}$th of an inch above it, and a small engraving placed between them. They were allowed to remain thus for six hours. When exposed to the vapour of mercury, the plate which had been subjected to the light whitened all over, and the space occupied by the engraving was distinctly marked by lines of vapour thicker than the other parts. The plate which had been preserved in the dark was scarcely at all influenced by the vapour, except on those parts which had been touched by the supports of cardboard on which it rested. These were so arranged that no radiation could have influenced those parts of the plates.

An iodized silver plate was placed in the dark with a little fine string coiled over parts of it, and a polished silver plate supported $\frac{1}{8}$th of an inch above it. After four hours both plates

were subjected to mercurial vapour. On the iodized plate the deposit of vapour was uniform, although slight; but on the superposed plate of silver a strong and beautiful image of the string on the under plate became visible. I found that neither of the two iodized plates had lost their sensitiveness by the operations to which they had been subjected in the dark.

Hoping to detect some evidence of the process by which these singular results were produced, I instituted a series of experiments, of which the following are some of the most interesting results.

A. A silver plate was iodized, a piece of card was placed upon it, and a well-polished mercurial plate (amalgamated copper) was suspended $\frac{1}{8}$th of an inch above it, and left in this state for a night. The space on the silver plate corresponding with the mercurial plate, except under the card, was nearly freed of its iodine, which had evidently combined with the mercury on the upper plate. On exposing the mercurial plate to the vapour of mercury the image of the card was rendered visible, the vapour covering every part of the plate except that opposite the card. The silver plate received the vapour only on those parts which were not influenced by the mercurial plate. The upper plate was suspended by strings; these were faithfully imaged on both plates; by a thick line of mercurial vapour on the under plate, by the absence of it in the upper one.

B. An iodized *silvered* plate was exposed to light until brown, and a mercurial plate suspended above it for twelve hours. The browned silver plate was *whitened*, and all the irregularities of the mercurial plate strikingly marked on it: the mercurial plate was slightly tarnished. On rubbing the silvered plate it was found that the silver was removed most readily over the whitened portion, but had lost none of its adhesion in other parts.

C. Over an iodized silver plate plates of gold, platina, silver, brass, copper, copper amalgamated, and zinc were placed at the distance of $\frac{1}{8}$th of an inch. After three hours the amalgamated plate had made a decided visible impression on the silver one. On exposure to vapour, the mercury lodged on every part of the plate but that affected by the mercurial plate; some irregularities were observed, but none which could be decidedly traced to the other metals in juxtaposition. I have some evidence that different metals near each other seriously interfere with each other's influence.

D. A mercurial plate was iodized, and another mercurial plate placed $\frac{1}{8}$th of an inch above it. The upper plate became

covered with a bright yellow film; and on exposing them to mercurial vapour, marks became apparent which corresponded with those in the opposite plate.

E. A silver plate was iodized and placed in the dark with an engraving, face down, upon it. An amalgamated copper plate was laid on this, and left for fifteen hours. The mercurial plate was reddened, and on exposure to the vapour of mercury, a very nice impression of the engraving was brought out, it having been effected through the thickness of the paper. On the silvered plate the space covered by the paper was well marked; but vaporization produced no trace of the engraving. The space beyond the paper was rendered white. It was curious that both plates had several spots which corresponded, particularly two, distinguished by a well-defined circle and a comet-like appendage, in length ten times the diameter of the circle. These spots could not be traced to anything visible in the print or either of the plates, and must, I think, be referred to some electrical influence. I find it indeed commonly the case, that the plates, after being subjected to these kinds of experiments a few times, become mottled, or present on their polished faces all the appearances of a finely-grained wood, and in this state they are less susceptible of receiving any impression than when not so.

F. A silver plate was iodized and placed upon an engraving laid on a brightly polished mercurial plate, and left in the dark for twenty-four hours. The mercurial plate was turned brown, and the silver plate was left in the same state as if it had been exposed to sunshine, being *brown and black*. Neither of these plates gave a copy of the picture.

G. A mercurial plate was iodized, and above it was placed a plate of polished iron, a disc of paper being first laid on the mercurial plate, and they were left in this state for some hours. On exposing the iron plate to mercurial vapour, it was abundantly lodged over that space opposite the paper disc, but not at all on the other parts. The mercurial plate was attacked by vapour over every part but that which the paper disc protected.

Lead and zinc plates were used instead of the iron one with nearly similar results.

H. A Daguerreotype view was taken, and without removing the iodine a mercurial plate was placed a little above it, and left for ten hours. When removed, well-defined traces of the Daguerreotype picture were evident on the mercurial plate, which leads me to hope that by careful manipulation we may succeed in multiplying these beautiful productions by an easy method.

I became desirous of ascertaining whether the mercurial plates would produce any change upon the precipitated iodide of silver. I find by many experiments, that if the iodide of silver is pure, no more change is produced than is produced upon it by diffused light; but if it is rendered sensitive by a trace of the nitrate of silver, it is then darkened as by solar influence.

Sensitive iodide of silver being placed upon a plate of glass, a mercurial plate was fixed $\frac{1}{8}$th of an inch above it. In three days the iodide of silver had become a deep brown, almost a black, and the mercurial plate was covered with the yellow iodide of mercury. Nitric acid dissolved the dark portion of the silver salt, as did also ammonia, on which was formed Faraday's oxide of silver, thereby proving the change, either by a primary or a secondary process, of the iodide into the oxide of silver. This experiment has been repeated at least a dozen times, and always with the same results. If a little heap of the iodide of silver is placed under a mercurial plate, it is exceedingly interesting to witness the gradual formation of the very beautiful coloured rings on the mercury in the progress of its conversion into an iodide. By prolonged action the yellow iodide passes into the bright red biniodide of mercury. I have some experiments now in hand, which convince me that similar chemical changes are to be effected through considerable spaces. I have succeeded in decomposing the iodide of copper and the iodide of gold by mercurial plates placed nearly a quarter of an inch above them.

I have an extensive record of results similar to those I have now detailed, all of them showing that the changes brought about by this mysterious agent, whether it be heat, light, or an undiscovered element, cannot be referred to those rays which the admirable researches of Sir John Herschel have shown to be the operative ones in producing the photographic phænomena which have so interested the world by their novel beauty, and which Professor Draper includes within his general term—tithonicity. With regard to the detithonizing influence of the gases mentioned by Dr. Draper in his paper in your March Number, I can only consider the results, which I find to be as he has stated, as the simple reconversion of the decomposed iodide of silver into another definite chemical compound. An iodized plate is exposed to light, the iodide of silver or other sensitive salt is decomposed, and in a state to receive mercurial vapour. It is now passed through an atmosphere of iodine, of chlorine, of bromine, or of nitrous gas. Chemists are well aware of the surprising energy with which these bodies attack the metals, consequently the ex-

posure of a moment is quite sufficient to convert the surface which has undergone a change, into an iodide, chloride, bromite, or nitrite of silver. I certainly cannot see the necessity of going so far out of our way for an explanation of this effect as Dr. Draper has done. I fear I have already occupied too much of your valuable space, or I might be inclined to trespass further. I shall however drop my pen for the present, again assuring you that I only desire to keep the image of Truth which is just shadowing our path, as free as possible from mists which might in any way obscure it.

I remain, Gentlemen,

Yours most obediently,

Falmouth, March 8, 1843. ROBERT HUNT.

L. *On Pyrogallic Acid, and some of the Astringent Substances which yield it.* By Dr. JOHN STENHOUSE*.

THE usual method of procuring pyrogallic acid is by cautiously distilling either gallic or tannic acids. The pyrogallic acid is obtained partly as a crystalline sublimate and partly dissolved in the empyreumatic liquor which passes into the receiver. Pyrogallic acid thus procured is very seldom free from empyreumatic oil, from which it can only be purified by repeated distillations, in each of which much acid is unavoidably lost. The usual method, therefore, of procuring pyrogallic acid by distillation is troublesome and unproductive. The process which I have found most advantageous for preparing it in quantity, is the following:—

Finely pounded gall-nuts are to be treated with successive portions of cold water till they are exhausted. These extracts are then to be evaporated and strongly dried, till all their hygrometric water is driven off. In this state they form a spongy deliquescent mass, in taste and colour very much resembling catechu. Instead of distilling this dried extract in a retort, it is much better to employ Dr. Mohr's apparatus for subliming benzoic acid. It consists of a cast iron pan from three to four inches deep, and from eighteen inches to two feet wide. The dried extract is coarsely pounded and spread equally over its bottom to the depth of about half an inch. The top of the pan is then covered with a diaphragm of bibulous paper, fitting closely over it by being pasted round its rim. The diaphragm may be pierced by a few small pin-holes, which greatly facilitates the sublimation. The pan is surmounted with a paper cap twelve or eighteen inches high, fitted closely to its top, and fastened by means of a cord passed two or three

* Communicated by the Chemical Society; having been read November 1, 1842.

times round it. The apparatus is to be cautiously heated for ten or twelve hours on a sand, or still better on a metallic bath. The temperature is to be kept as nearly as possible at about 400° F., though towards the end of the sublimation it may be raised a few degrees higher. The crystals of pyrogallic acid pass up through the bibulous paper, which absorbs the empyreumatic oil by which they are always accompanied. Should the heat have been carefully regulated, the crystals, which are either large scales or needles, are perfectly white; should they be slightly coloured, which sometimes happens, they may be easily purified by a second sublimation. This method possesses the great advantage that a pound or more of the extract can be operated on at once; the apparatus is extremely cheap, and as it is not liable to break it may be used for any number of times. On one trial 1380 grains of dried extract yielded 69 grs. of perfectly pure crystals, and 74 grs. which were slightly coloured, in all 143 grs., or 10·3 per cent. Now as galls yield rather more than half of their weight of soluble matter, the quantity of pyrogallic acid obtainable from them by this process is very considerable. I think it right to mention, however, that on a previous trial, when the sublimation was not so carefully conducted, I did not obtain more than half this quantity.

The following are some of the leading characters of pyrogallic acid. It has a very bitter taste, resembling that of salicine. When pure it does not redden litmus paper; but if it has been sublimed at too high a heat, it is accompanied with a little of some volatile acid, which causes it to redden litmus slightly. It gives a deep indigo-blue colour with solutions of protosulphate of iron, but no precipitate falls. If the protosalt contains any peroxide, this colour soon changes to dark green; but if the salt is pure the deep blue colour remains for a considerable time. With persulphate of iron it gives a yellowish red; with perchloride a much brighter red, but in neither case any precipitate. When pyrogallic acid is dropt into milk of lime, a beautiful reddish purple colour appears, which however speedily changes to a dark brown. Caustic barytes produces a dark brown colour which quickly becomes black. Its reactions with salts of iron and milk of lime are the best tests for pyrogallic acid, and by these its presence even in very small quantity can be easily ascertained. It is very soluble in water, but its aqueous solution, if exposed to the air, speedily blackens. It is also very soluble in alcohol, though not so much so as in water. The taste of its alcoholic solution very much resembles laudanum. Dilute sulphuric acid first reddens pyrogallic acid and then blackens it. Iodine has no effect upon it. Dry chlorine instantly colours the crystals

of pyrogallic acid bright red and then blackens them. Moist chlorine, when sent through a solution of pyrogallic acid, gives it a hyacinth-red colour, and evolves much muriatic acid; no precipitate fell, however, and when left to spontaneous evaporation it yielded no crystals, but left only a reddish gummy mass. Pyrogallic acid reduces the oxides of gold, silver and platinum to the metallic state, and precipitates them completely from their solutions. In order to test the purity of the pyrogallic acid I had obtained, it was dried at 212° F., and analysed in the usual way. 0·312 gram. gave 0·65 carbonic acid, and 0·1345 water.

Found.		Calculated.		
C 57·60	C	8 =	611·480 =	57·61
H 4·78	H	4 =	49·918 =	4·70
O 37·62	O	4 =	400·000 =	37·69
100·00			1061·398	100·00

This result agrees closely with the calculated numbers given above.

I next endeavoured to form the hydrate of pyrogallic acid by dissolving some very pure crystals in a small quantity of water, and evaporating the solution *in vacuo* over sulphuric acid. It gave large white needles of a silky lustre.

Burnt with oxide of copper,—

I. 0·3265 gramme substance, dried *in vacuo*, gave 0·68 carbonic acid, and 0·138 water.

II. 0·2873 gramme, dried at 212° F., gave 0·600 carbonic acid, and 0·124 water.

	I.	II.	Calculated.
C	57·58	57·83	57·61
H	4·97	4·79	4·70
O	37·45	37·38	37·69
	100·00	100·00	100·00

Pyrogallic acid, therefore, does not form a hydrate.

Pyrogallate of Lead.

In order to determine the atomic weight of pyrogallic acid, the lead salt was prepared by adding a solution of pyrogallic acid to an excess of neutral acetate of lead in the cold. A copious white flocculent precipitate immediately fell. It was washed repeatedly by decantation, then thrown upon a filter and rapidly washed, the air being excluded as much as possible, and when pressed between folds of blotting-paper was dried *in vacuo*. When dried it was still nearly white, having only a slight shade of yellow.

I. 0·7985 gramme, dried *in vacuo*, gave 0·269 oxide of lead and 0·174 metallic lead = 57·16 per cent. oxide.

II. 0·731 salt gave 0·252 oxide and 0·153 lead = 57·02 oxide of lead.

III. 0·6402 gave 0·1712 oxide and 0·1815 lead = 57·28 per cent. oxide.

IV. 0·709 salt, dried at 212° F., gave 0·252 oxide and 0·143 lead = 57·27 per cent. oxide.

0·5685 salt gave, when burnt with oxide of copper, 0·504 carbonic acid and 0·117 water.

		Calculated.	
C = 24·51	8 C =	611·480 =	24·89
H = 2·28	4 H =	49·918 =	2·03
O = 16·03	4 O =	400·000 =	16·30
Pb O = 57·18	Pb O =	1394·500 =	56·78
100·00		2455·898	100·00

The mean of these determinations gives 1044 for the atomic weight of pyrogallic acid, which corresponds pretty closely with the calculated number 1061. It is evident also that these analyses give C 8 H 4 O 4 as the formula of pyrogallic acid; and not $C_6 H_3 O_3$, that of Berzelius. The formula $C_8 H_4 O_4$ is that found by the late R. C. Campbell, as stated in Liebig's Geiger; but I am not aware that the details of his analysis have ever been published.

When even a single drop of caustic ammonia is added to a solution of pyrogallic acid, it becomes alkaline and assumes a dark brown colour. A great excess of ammonia was added to a solution of pyrogallic acid, which was then evaporated *in vacuo*. The acid crystallized in confused tufts of a dark brown colour; when pounded, however, its colour was only light brown. It did not give the least indication of ammonia when heated either with a strong solution of potash or with quick lime. When dried *in vacuo* and subjected to analysis,

I. 0·4620 substance gave 0·9255 carbonic acid, and 0·201 water.

II. 0·3141 substance, also dried *in vacuo*, gave 0·6314 carbonic acid, and 0·1405 water.

	I.	II.
C	55·38	55·58
H	4·83	4·96
O	39·79	39·46
	100·00	100·00

Now pyrogallate of ammonia should have given only 44·44 per cent. carbon, and 6·34 per cent. hydrogen.

The substance of number II. was prepared at a different time from number I.; and in its preparation a still larger excess of ammonia was employed. The reactions of the supposed pyrogallate of ammonia with salts of iron and milk of

lime were exactly the same as those of ordinary pyrogallic acid. It is evident, therefore, that pyrogallic acid does not combine with ammonia, but is slightly oxidized when brought in contact with it.

The addition of a little potash also rendered solutions of pyrogallic acid alkaline and even darker coloured than ammonia does. The coloration takes place first at the surface of the liquid, and is evidently the effect of oxidation. When evaporated *in vacuo* it became a black gummy mass, which showed no disposition to crystallize. When this black mass was dissolved in water, in which it is very soluble, and treated with sulphuric acid, it effervesced, apparently from the escape of carbonic acid. It also gave off the vapours of acetic acid in abundance. These were easily recognisable by their smell, and by their immediately reddening litmus paper when held over them. When the solution was very concentrated, a little of a dark brown matter precipitated; but if the solution was at all dilute no precipitate appeared. I made an unsuccessful attempt to collect this precipitate on a filter, and to free it from adhering sulphuric acid by washing it with cold water. The whole of the black matter was speedily dissolved and passed through the filter. Soda produced similar effects on pyrogallic acid. It is plain, therefore, that pyrogallic acid is decomposed by the alkalies, but does not combine with them, and that its acid properties, if indeed it possesses any, are very feeble. In this and some other respects it closely resembles pyromeconic acid.

When pyrogallic acid is dropped into acetate of copper, it causes a dark brown precipitate, which however quickly becomes black, and is very soluble. When we attempt to collect it on a filter and to wash it, the greater portion of the salt is dissolved by the water. The liquid when it first passes through the filter is colourless, but on standing a few minutes it becomes dark brown and slowly deposits a new precipitate. The compound first formed appears to be decomposed by the water.

When pyrogallic acid is added to a solution of bichromate of potash, it immediately turns it yellowish brown, and then dark brown till the liquid is almost opake, but no precipitate falls.

LI. *On a new Method of obtaining pure Silver, either in the Metallic State or in the form of Oxide. By* William Gregory, *M.D., F.R.S.E., Member of the Chemical Society, &c.**

THE chemist, as well as the metallurgist, has frequent occasion to purify silver, especially from copper, which is dissolved along with it by nitric acid, the proper solvent of

* Communicated by the Chemical Society; having been read February 7, 1843.

silver. By converting the silver into the insoluble chloride, it is effectually purified from copper as well as from all other metals, the chlorides of which are soluble. But here the difficulty begins : the chloride of silver is a very unmanageable product, at least in the moist way. It is true that if placed in water acidulated with hydrochloric acid, in contact with zinc or iron, the chloride of silver is reduced. But the process is tedious, seldom complete, and in the end unsatisfactory; for some zinc adheres to the reduced silver, so that it is not removed by digestion with moderately strong hydrochloric acid. This is proved by the action of ammonia, which extracts a good deal of oxide of zinc. Moreover, the zinc or iron is hardly ever pure ; and its impurities, arsenic, carbon, and perhaps also copper and tin, remain with the silver. I have never got from silver thus reduced, a colourless solution of nitrate.

It is no doubt better to decompose the dried chloride of silver by the action of carbonate of potash or soda at a red heat. But although the silver is thus obtained pure, the process requires much experience and dexterity. If the heat be too low, the reduced silver is disseminated in small globules through the mass ; if too high, the alkali corrodes the crucible rapidly, and the contents fall into the fireplace or ash-pit. There is often also a portion of silver cast up by the effervescence on the sides of the crucible, in small globules, which do not readily run down into the fused mass below. In short, this process, always ticklish, often fails. It is therefore desirable, if possible, to dispense with a furnace heat.

The method of reducing the silver from the impure nitrate by protosulphate of iron does not answer. It is long ere the action is terminated, and besides, some sulphate is always formed, which *is not reduced*, and is partly precipitated with the metal, and partly retained in solution.

The only remaining method, known to me, is that of reducing the silver from the impure (cupreous) nitrate or sulphate, by means of copper. The chief objection to this method is that it is somewhat tedious; but it is also not improbable that a trace of copper may adhere to the silver, chemically combined, as is the case to a large extent with mercury in the *Arbor Dianæ*. At least I have generally found copper in the silver I have thus prepared.

Considering these things, it appeared desirable to have once more recourse to the chloride, which can be easily obtained perfectly pure, and to decompose it without the contact of any reguline metal. The most obvious plan was to try the action of caustic alkalies in the moist way, and although it has been singularly enough hitherto overlooked, I find that caustic potash may be used with complete success. Diluted

potash, or even a concentrated solution, if cold, has no apparent action on chloride of silver; and this, I presume, explains how the reaction about to be described has not been noticed. But a solution of potash, spec. grav. 1·25 to 1·30, with the aid of heat, decomposes almost instantaneously the moist chloride of silver, converting it into a heavy, fine jet-black powder, which is pure oxide of silver. This oxide dissolves without the smallest residue, and without effervescence in diluted nitric acid, and yields a colourless and pure nitrate. The heat of the spirit-lamp reduces the oxide to a coherent spongy mass of absolutely pure silver.

The following method appears to me the most advantageous.

The cupreous solution of silver is precipitated by common salt, while hot, and the chloride of silver well washed by decantation with hot water. It should also be broken down with a spatula of platinum or a glass rod, during the washing, but not ground in a mortar, which causes it to cake, and impedes the action of the potash. The chloride, *while still moist*, is covered to about half an inch with a solution of caustic potash, spec. grav. 1·25 at least, and then boiled. During the boiling, which is best performed in a capsule of clean iron, silver or platinum, the chloride is to be well stirred, in order to bruise all curdy or lumpy particles. In five or ten minutes the powder has become black. If a small portion, taken out and washed, do not dissolve without residue in dilute nitric acid, the potash is to be decanted off, and the powder, still moist, is to be well rubbed down in a mortar, which may *now* be done with advantage. It is then returned into the capsule, and again boiled for five minutes with the same, or with fresh potash. It will now dissolve entirely in nitric acid; but if not, a second grinding will infallibly succeed. It is now only necessary to wash the oxide, which is completed by decantation in a few minutes, as the powder, from its great density, sinks at once to the bottom. The first two or three washings are made with hot water, the remainder with cold water; for when the oxide is nearly washed, it rises partially to the surface, *with hot water*, and thus a loss is occasioned in decanting. Of course, the whole washings (except the first, owing to the strength of the potash) may be conducted on a filter. But the powder is so fine, that probably a good deal would adhere to the paper when dry.

This oxide of silver appears in a form quite distinct from that of the oxide precipitated by potash from the nitrates, and is hitherto undescribed. It is very dense, homogeneous, and has a pure black colour, which has, if anything, a tint of blue; whereas the common oxide is bulky, far less dense, and of a grayish brown colour. They appear, however, to be chemi-

cally identical. Not having a microscope, I have not studied their physical characters minutely; but I suspect, from its aspect in the liquid in which it is formed, that the new oxide is crystalline.

It is obvious that the above process furnishes an easy method of procuring a very pure oxide of silver, and of course the action of heat gives us the silver in the state of metal. It is, I conceive, applicable both to the manufacture of nitrate (in a state of absolute purity) and to the metallurgic process for obtaining pure silver. For both objects, it is a matter of no consequence, if some chloride should have escaped the action of the alkali. This chloride is left undissolved by the nitric acid, and is separated by filtration; while if the oxide (not quite free from chloride) be mixed with a little nitre or carbonate of potash, and fused, the whole silver is obtained with the utmost facility*. In order to give an idea of the ease with which the whole is performed, I may mention that I dissolved a half-crown, and obtained the whole of the silver it contained, within a very trifling fraction (chiefly decanted in the *first* washing of the chloride, *but not lost*), by the above process, *within two hours*, in a fused state. The silver was quite pure. There is no doubt that to chemists also an easy method of obtaining quickly pure oxide of silver, in a form much less hygrometric than the usual one, will be acceptable.

It is particularly to be noticed, that if the chloride have ONCE BEEN DRIED, it is with great difficulty decomposed, even by a long boiling with potash.

King's College, Aberdeen, Jan. 20, 1843.

LII. *Observations on* M. Reiset's *Remarks on the new Method for the Estimation of Nitrogen in Organic Compounds, and also on the supposed part which the Nitrogen of the Atmosphere plays in the formation of Ammonia. By* H. WILL, *Ph. D.*†.

THE method for the estimation of the nitrogen in organic substances described by Varrentrapp and myself‡ has been received by many chemists with the greatest approbation, as well on account of its simplicity as the accuracy and security with which results can be obtained. M. Reiset has however

* In fact, this process, imperfectly performed, is an excellent preliminary step, when a large quantity of chloride is to be reduced. The impure oxide requires so little alkali to complete its decomposition, that the crucible runs no risk. A little borax may be added as a flux.

† Communicated by the Chemical Society; having been read March 21, 1843.

‡ *Annal. der Chemie*, b. xxxix. s. 257. See also Philosophical Magazine for March 1842, p. 216.

presented an essay to the Academy of Sciences at Paris*, in which he endeavours to prove by experiments, at first sight very convincing, that the above-mentioned method is attended by two sources of error; the first of which in particular, were it true, would be quite sufficient to destroy completely the value of the method.

The cause of this first and principal source of error is, according to M. Reiset, that the nitrogen of the atmosphere forms a portion of the ammonia produced by the decomposition of nitrogenous matter by means of an alkaline hydrate, and that consequently too large an amount of nitrogen must always be obtained, particularly in bodies rich in carbon, bodies of difficult combustion, and those which readily form cyanogen compounds. This source of error becomes the more important, as from the experiments of Faraday †, which are confirmed by Reiset, it appears that by the fusion of many non-nitrogenous bodies with the hydrates of the alkalies a pretty considerable quantity of ammonia is formed. To those non-nitrogenous bodies which produce ammonia belongs in particular sugar, a substance which we proposed as an addition for the purpose of diminishing the violence of the absorption of the ammonia by the hydrochloric acid.

The numerous analyses of nitrogenous bodies made by Varrentrapp and myself, must have given a very considerable excess of nitrogen, if any formation of ammonia really took place, and were a constant source of error: it would be particularly evident in the analyses of ammeline, in which we mixed the substance with an equal weight of sugar. The accuracy and strictness of the results obtained by us from substances of well-known composition could therefore be ascribed only to accident, or perhaps to some source of error balancing the one just mentioned.

We thought we had met every objection of a source of error on this point by the experiments mentioned in our paper, in which we passed nitrogen and hydrogen gases through a glass tube over a red-hot mixture of carbonized bitartrate of potash and lime, or of pure charcoal soda and lime, and from which we did not obtain sufficient ammonia to be estimated as ammonio-chloride of platinum; and yet all the conditions necessary for the formation of ammonia and cyanogen were afforded in the mixture of soda, lime and carbon by the hy-

* *Compt. Rend.*, vol. xv. p. 154; and *Annal. de Chim. et de Physique*, 3rd ser. vol. v. p. 469.

† Quarterly Journal of Science, vol. xix. p. 16; and Poggendorff's *Annalen*, vol. iii. p. 455. [Also Phil. Mag. S. 1. vol. lxv. p. 309.]

drogen becoming free from the combustion of the carbon at the expense of the oxygen of the hydrated water, as well as through the difficulty of its combustion.

M. Reiset appears to have overlooked the fact that finely divided carbon is also, as well as an organic substance, oxidized completely by means of the hydrates of the alkalies, and states that we have neglected to prove in a satisfactory manner, that the facts observed by Faraday, according to which non-nitrogenous bodies, as sugar, acetate of potash, oxalate of lime, tartrate of lead, &c., by ignition with potash, soda, and hydrate of barytes and access of air give an appreciable quantity of ammonia, are without influence on the new process of analysis. He has undertaken this for us; and his experiments, which were made with stearine and sugar, gave him on combustion with soda-lime, under the same circumstances as in the execution of a nitrogen analysis, the following very remarkable results:—

Sugar.	Platinum obtained.	Nitrogen obtained.	Nitrogen in 100 parts.
0·250	0·02650	0·0038	1·52
0·500	0·05250	0·0075	1·50
1·000	0·0890	0·0127	1·27
1·500	0·104	0·0149	1·00
2·000	0·10725	0·0153	0·75

In these experiments the quantity of ammonia obtained was in exact proportion to the quantity of sugar employed, as far as *one* gramme; with *more* sugar more ammonia was not obtained.

Reiset obtained further from 1 gramme stearine, 0·06475 platinum = 0·0092 nitrogen, and in two other experiments with sugar performed in an atmosphere of hydrogen (from 1 gramme), 0·03375 and 0·034 platinum = 0·0048 nitrogen.

From both these last experiments, according to which non-nitrogenous bodies also eliminate ammonia in an atmosphere of hydrogen, M. Reiset concludes that the alkaline mixture possesses the property of condensing nitrogen so intimately and strongly that it cannot be expelled completely by a current of hydrogen passed over it for six hours, and that this state of condensation, approaching as it does the nascent state, makes the nitrogen more apt to enter into combination.

As a further proof of the incorrectness of our method, M. Reiset brings forward the analysis of cinchovatina, an organic base discovered by Manzini in Jaën Cinchona, from an analysis of which, performed with a mixture of sugar, almost 5 per cent. more of nitrogen than the calculation required was obtained. 0·052 cinchovatina gave, namely 0·949 ammonio-chloride of platinum = 11·95 per cent. nitrogen.

The calculation from the formula $C_{46} H_{27} N_2 O_8$ gives only 7·16 per cent.

The excess of 4·8 per cent. here obtained, estimated by weight, amounted to 0·024 gramme nitrogen, or in volume nearly 25 cubic centimetres; in the above experiments with sugar 0·015 gramme of nitrogen was, according to M. Reiset, condensed in the soda-lime, and therefore took a part in the formation of the ammonia.

If we consider that the decomposition of organic bodies of difficult combustion by the hydrates of the alkalies does not take place at a heat below redness, that further, the heating of the contents of the tube by the fire placed around it cannot be so sudden as to produce in an instant the temperature necessary for combustion, but that the heat, even when sudden, penetrates the mixture only by degrees, and that the greater portion of the inclosed or condensed air is driven out by its own expansion, we can scarcely comprehend how M. Reiset could entertain the idea that the nitrogen condensed in the mixture could take a part in the formation of ammonia. He certainly brings forward an experiment apparently supporting this view, viz. that by the combustion of 1·500 gramme of sugar in a current of air, the combustion being thus quickened, only 0·0099 nitrogen was obtained, instead of 0·0149. The ammonia did not increase when pure nitrogen was passed over the mixture during the combustion. I shall subsequently return to this point.

I have repeated and partly varied the experiments of Reiset, and have come to entirely different results.

1·214 sugar-candy of the shops by combustion with the usual mixture of soda-lime, which had not been previously ignited, gave on evaporation with chloride of platinum and ignition of the washed residue, 0·006 metallic platinum = 0·00086 nitrogen = 0·07 per cent. of the sugar burned.

0·386 pure stearic acid recrystallized from alcohol, gave 0·002 metallic platinum = 0·00028 nitrogen.

0·430 leguminous starch, prepared in the laboratory of Giessen, and purified with sulphuric acid, gave 0·005 metallic platinum equivalent to 0·0007 nitrogen.

A gramme of the above-mentioned starch was submitted to dry distillation. The product of distillation was mixed with hydrochloric acid, evaporated, the residue dissolved in water, mixed with chloride of platinum, and again evaporated. After treatment with alcohol and æther, a portion of ammonio-chloride of platinum remained, which ignited left 0·004 metallic platinum. The ammonia obtained by the combustion with soda-lime was thus, in part at least, contained in the starch, and was no product of the operation.

In both the following experiments, conducted exactly as an ordinary combustion, I employed soda-lime ignited just before its introduction into the tube.

1·000 gramme stearic acid decomposed with soda-lime in a tube $1\frac{1}{2}$ foot long and half an inch wide, left, after evaporation to dryness with chloride of platinum and resolution in æther-alcohol, no visible trace of ammonio-chloride of platinum.

2·000 grammes pulverized metallic tin, after ignition with soda-lime and treatment of the residue after the evaporation of the hydrochloric acid with chloride of platinum, afforded an extremely small quantity of yellow powder, which possessed all the properties of ammonio-chloride of platinum.

In the following experiments, a stream either of atmospheric air or of nitrogen was passed through the tube during the successive oxidation of the substance, by means of an alkaline hydrate. Both the atmospheric air and the nitrogen were dried by means of sulphuric acid, which had been freed from nitric oxide by treating with sulphate of iron.

The volume of gas passed through was about from three to four thousand cubic centimetres, and the combustion throughout the whole length of the tube was so conducted, the experiment lasting from two to three hours, that the conditions necessary for the formation of ammonia were given at every moment.

4·000 grms. perfectly pure recrystallized sugar, ignited in a tube $2\frac{1}{2}$ feet long, with a large mass of soda-lime in a current of air, gave no trace of ammonio-chloride of platinum.

20 grms. of common pulverized tin, oxidized in the same way with soda-lime, gave a quantity of yellow powder, too small to be weighed. The uninterrupted disengagement of hydrogen proved, however, that the tin was oxidized at the expense of the alkaline hydrate.

4·300 grms. of recrystallized sugar were introduced by degrees through the tubulure of a retort whose neck was obliquely turned up, and in which soda-lime was in a state of fusion. An aspirator was attached to the absorption apparatus connected with the retort, so that the gaseous product formed, following the current of air, should pass through the hydrochloric acid. Only an extremely small trace of ammonio-chloride of platinum was obtained. The same experiment repeated with tin, zinc, and pure metallic iron, always afforded ammonio-chloride of platinum, yet so slight a trace that in most cases it did not admit of being estimated.

When hydrate of potash was employed instead of hydrate of soda, I always obtained potassio-chloride of platinum, because, from the violent evolution of the hydrogen, portions of the alkali were driven over into the hydrochloric acid.

In another experiment 20 grms. of metallic tin were melted with fresh fused hydrate of soda in a thin U-formed tube, with access of atmospheric air, so that during the continuance of the experiment a fresh quantity of air was always brought into contact with the nascent hydrogen; I obtained thus 0·008 ammonio-chloride of platinum = 0·00057 nitrogen. In an experiment in which nitrogen was used instead of atmospheric air, a similar result was obtained, namely 0·007 ammonio-chloride of platinum.

These experiments prove that the nitrogen of the atmosphere can in no way form ammonia with hydrogen in a nascent state. The extremely small quantities obtained in most cases must consequently have some other source, which it is very difficult to avoid. This however may be attained by the following method:—

Hydrate of soda was melted in a silver crucible until it became liquid, and then mixed with a small quantity of pure iron, reduced from the oxide by means of hydrogen. This was readily oxidized with disengagement of hydrogen gas; it was then poured into a silver dish previously warmed, and after it had cooled was broken into pieces and introduced into a slightly curved tube of hard glass half an inch in diameter, previously ignited; from 4 to 5 grammes of pure iron reduced from the oxide by hydrogen, were then immediately added; the tube was heated by charcoal placed under it, and nitrogen or atmospheric air passed through it. On the first passage of the air an extremely small quantity of ammonia was generally detected by means of dahlia-paper, or by a rod moistened with dilute hydrochloric acid; but this disengagement of ammonia was only observed for a short time, and always ceased before the evolution of the hydrogen, from the oxidation of the iron, began. When this took place it was connected with the absorption apparatus, and the alkali kept in a state of fusion until all the metal was oxidized. By carefully following this plan I never obtained ammonio-chloride of platinum.

The same experiment was repeated with a like result with perfectly pure crystallized tin, as it is easily obtained when a polished rod of tin is suspended in a vessel in which water with a little hydrochloric acid, rests on a concentrated solution of tin; after one or two days a splendid crystallization forms. If the metal happened to be touched by the fingers, or allowed to remain exposed to the air before the experiment, a disengagement of ammonia invariably occurred; but not when it as well as the alkaline hydrate were fused just before being employed. Pure tin is with great difficulty oxidized by

hydrate of soda, and must be kept in a state of fusion with it for many hours before the oxidation is complete.

Reiset states that ammonia is disengaged by heating metallic iron and potash-ley to 292° Fahr., with access of air, but not so in an atmosphere of nitrogen. This statement rests on a very equivocal foundation. Pure iron can be heated for a long time in a boiling potash-ley without the disengagement of hydrogen; the oxidation takes place only on the fusion of the alkaline hydrates. If a quantity of potash-ley which has stood for a long time in a perfectly clean retort be heated, there is always observed at the commencement a slight disengagement of ammonia, but this soon ceases altogether.

The, by no means inconsiderable, quantities of ammonia obtained by Reiset, admit of no other explanation than that his mixture of soda and lime contained a nitrate, probably nitrate of potash, which, as Faraday states, easily evolves ammonia when the smallest trace of it is melted with zinc and an alkaline hydrate. If Reiset had only, in a small degree, followed or observed the extremely cautious and circumspect manner of proceeding of that celebrated English philosopher, a manner which is with justice admired by him, he would not have endeavoured to find sources of error in a method[1] to which, on this point at least, no very weighty objection can be made.

The nitrate of potash contained in the soda-lime used by Reiset, was very probably owing to the circumstance that most manufacturers add a little of it to the commercial hydrates of soda and potash, for the purpose of improving their appearance. In Reiset's experiment, where he obtained 4·8 per cent. too much nitrogen in chincovatina, his mixture must have contained very nearly $\frac{1}{2}$ per cent. of nitrate of potash, when it is considered that his tube contained from 50 to 60 grammes. This also explains in a much simpler and easier manner the formation of ammonia in an atmosphere of hydrogen, and also the limited increase of ammonia from the increased quantities of sugar employed. As the whole quantity of nitrate of potash would be destroyed by from 1 to $1\frac{1}{2}$ gramme of sugar, the quantity of ammonia could not increase. The nitrogen was here certainly contained in such a condensed state, that a stream of hydrogen gas passed over it during twelve hours did not expel it.

I now come to the second source of error objected, by M. Reiset, to the new method. It appears from his statements, that a part of the chloride of platinum is reduced to protochloride when the hydrochloric acid fluid, which in many cases contains liquid hydro-carburets, is evaporated to dry-

ness with it in a water-bath; consequently too much nitrogen must always be obtained, as this protochloride of platinum is insoluble in æther and alcohol. And this source of error has the more injurious effects on the result the more its necessary conditions are afforded, and these conditions are the blackening of the hydro-carburets by the hydrochloric acid. In a direct experiment with sugar made for this purpose, and in which the burning was managed in such a manner that the hydro-carburets being produced at a low temperature floated in abundance on the hydrochloric acid, on evaporation in a water-bath no reduction of the chloride of platinum could be observed. It must be allowed, that in such a trifling case of occurrence, the result would not be affected by it. Indeed, the formation of hydro-carburets, easily decomposable by hydrochloric acid, may be completely avoided by keep ing the nearer end of the tube pretty strongly ignited, as the hydro-carburets are the more constant when produced at high temperatures.

The highly remarkable and accurate experiments of Faraday on the disengagement or formation of ammonia by the fusion of hydrate of potash with a metallic or a non-nitrogenous body, a result which I have also found in all my experiments (but which was so trifling that it could not be attributed to any part played by the nitrogen of the atmosphere), as also the investigation of Professor Liebig on the ammonia contained in rain-water, gives a complete and simple solution of the question, from whence comes this disengagement of ammonia so often observed and so difficult to be avoided?

The experiments of Faraday go entirely to show that there is some unknown source of ammonia, and that the nitrogen of the atmosphere in his experiments played no actual part; they are so convincing and made without any preconceived opinion, that I cannot refrain from giving a short extract from them here. They prove, as it appears to me, directly the reverse to the conclusion which M. Reiset has drawn from them, and are of the greatest importance in the question, whether the nitrogen of the atmosphere plays a temporary part in the formation of ammonia by the decay of organic matter, or by the oxidation of metals with or without the disengagement of hydrogen? An affirmative or negative to this question has a very important influence on the theory of the nutrition of plants.

Faraday observed that an organic substance, the quantity of whose nitrogen he wished to estimate, yielded ammonia by fusion with hydrate of potash, although he obtained none when it was heated alone in a tube. By extending his expe-

riments further, he found that many non-nitrogenous organic bodies, as also many metals, presented this phænomenon, as for instance, iron, zinc, tin, lead, arsenic, and also copper. He obtained, for example, a very perceptible quantity of ammonia with woody fibre, oxalate of potash, oxalate of lime, tartrate of lead, acetate of lime and asphaltum; with acetate of potash, acetate and tartrate of lead, tartrate and benzoate of potash, oxalate of lead, sugar, wax, olive oil and naphthaline, very little; and with resin, alcohol, æther and olefiant gas, none whatever. The quantity of ammonia agreed in a remarkable manner with the quantity of hydrate of potash used in the experiment. He further observed, that perfectly pure hydrate of potash, evaporated so far that it ceased to give off water, when heated alone yielded no ammonia, but that it acquired this property when exposed to the air for some time. He observed exactly the same with caustic lime and hydrate of lime, and also with fresh prepared potash-ley allowed to stand for twenty-four hours.

Faraday further obtained ammonia when he heated a strip of well-purified zinc with hydrate of potash made from potassium, in a carefully prepared atmosphere of hydrogen; but could discover no ammonia when he heated the zinc with hydrate of potash which had been previously kept in a state of fusion until it ceased to give off water. He states, moreover, that the ammonia was generally observed before the disengagement of the hydrogen by the decomposition of the substance employed commenced.

Tartrate of lead ignited with potash and the cold residue brought in contact with a drop of water, evolved ammonia.

White clay from Cornwall, which after being strongly ignited was exposed to the air for eight days, yielded much ammonia, while another exactly similar portion of the same clay, which after ignition was preserved in a well-stoppered bottle, gave no ammonia.

Pure sea sand, heated to bright redness in a crucible and cooled on a plate of copper, gave no trace of ammonia, although it was very readily observed when the hot sand previous to its being heated was held for some moments in the hand and stirred about with the finger.

These experiments evidently agree with the observations of Braconnot*, who states that many porous minerals, such as trap from Chaume de Tendon, eurite, some species of granite, serpentine from the Vosges, amphibole, muschelkalk, &c., by distillation in a glass retort, yielded an ammoniacal product.

* *Annales de Chimie et de Physique*, t. lxvii. p. 104.

The experiments of Faraday show with the greatest accuracy that the ammonia was not only not formed, but that it either existed already in the material employed, or received it from the air by exposure. The quantities obtained were so extremely small that he could not estimate them.

In the foregoing experiments I have not only confirmed, but at the same time demonstrated the correctness of Faraday's statement, that the nitrogen of the atmosphere does not in any way possess the property of forming ammonia with hydrogen at the moment of its separation from any combination. If this were the case, a quantity of ammonia capable of being estimated, and in proportion to the duration of the experiment or the quantity of the material, would have been obtained in the experiments with tin, iron and sugar, in which, by the gradual heating of the substance with an alkali in a continued current of air or of nitrogen, the conditions for the formation of ammonia were as favourable as possible throughout the whole combustion; but this did not occur, and by proper care we are even in a condition to avoid every trace of ammonia, although nascent hydrogen may come in contact with nitrogen gas.

If we consider that ammonia forms a never-failing constituent of our atmosphere, that further, it is a body which is easily absorbed by liquid and porous substances, particularly when these latter possess at the same time the properties of an acid, we must at once perceive that, being in possession of an exceedingly delicate test of the presence of ammonia, that volatile alkali must be found in all, or nearly all substances exposed to the air.

It is quite evident from this why Faraday did not obtain ammonia with fresh hydrate of potash which had been previously melted, nor with resin which is not a porous body, although resin, like other organic bodies, was decomposed with the disengagement of hydrogen gas by fusion with the hydrate. A small quantity of nitrogen contained in the body as a constituent may be in part or altogether the cause of the disengagement of ammonia in many cases where Faraday observed it. The fact that the flocculent black residue always obtained by the solution of zinc in sulphuric acid after being well washed disengages a pretty considerable quantity of ammonia, accounts very easily for the [presence of nitrogen in commercial zinc. Cast iron, according to Schafhaeutl, also contains nitrogen.

The statements contained in most treatises on chemistry, that iron by its change into oxide under the combined influence of moisture and air containing carbonic acid affords the nitrogen of the latter the conditions necessary to form ammonia, agree exactly with the above cases of its supposed formation. This production of ammonia, if it actually took place, presupposes that iron is capable of decomposing water

with the disengagment of hydrogen gas at the common temperatures, which is by no means the case; it presupposes further, that the hydrogen on being set free possesses a far greater affinity for the nitrogen than for the oxygen of the atmosphere, which completely contradicts our general experience. At high temperatures, where water would be decomposed by iron, ammonia is not formed. Kuhlman* obtained only hydrogen and nitrogen, but no ammonia, by the passage of steam, and nitrogen over pyrophorous iron heated to a strong red heat.

I have repeated the doubtful experiment of Austin (at least according to the result of Hall †), in such a manner that the ammonia contained in the atmosphere (but not its carbonic acid) was as perfectly as possible shut out. I introduced into a flask of from 4 to 5 litres capacity, some iron nails (one pound) previously cleaned from all oxide by dilute hydrochloric acid, and then well washed with pure water, and also sufficient distilled water to cover the bottom. The flask was connected air-tight by an intermediate tube with a second smaller one, which contained a small quantity of very dilute muriatic acid. By a second hole bored in the cork of the small flask, a tube containing asbestus moistened with pure sulphuric acid was attached, and through which the external air communicated with that contained in the greater flask. The object of the hydrochloric acid was to prevent the ammonia formed from passing into the sulphuric acid. The air was renewed every day in such a manner through a second tube closed with wax in the cork of the first flask, that the air entering must pass through the sulphuric acid tube.

After from 14 to 18 days oxidation, the oxide, of which a considerable quantity had already formed, was washed out of the flask with water and a little dilute hydrochloric acid; dissolved in hydrochloric acid and the solution to which chloride of platinum was added, evaporated nearly to dryness in a water-bath. The residue dissolved completely in æther-alcohol, and did not deposit a trace of ammonio-chloride of platinum even after standing for twelve hours, nor could any ammonia be found in the muriatic acid contained in the small flask. If the iron was here oxidized at the expense of the water, and if the hydrogen by that means set free had at the moment of its disengagement formed ammonia with the nitrogen of the air, nearly 3 grammes of ammonio-chloride of platinum would have been obtained for every gramme of the oxide treated in the above manner. This is a quantity which could not escape observation.

* *Abhandlung ueber die Saltpeterbildung*: *Annal. der Chem. und Pharm.*, Bd. xxix. S. 285.

† *Ann. de Chim. et de Phys.* t. ii. p. 42.

There is no doubt from this that the ammonia observed in the rust of iron was obtained from the atmosphere.

Herman*, in an essay "On the decay of wood," mentions an experiment in which the nitrogen of the atmosphere was directly absorbed and partially converted into ammonia by the decay of fresh wood. Herman found nearly one-third part of the nitrogen, which according to his experiments existed as a constituent of the wood, in the products of its decay. He concludes from this that two parts escaped in the form of ammonia.

The most perfect process of decay with which we are acquainted is the production of acetic acid from alcohol. If the nitrogen of the atmosphere possessed the property of taking part in these metamorphoses instead of pure acetic acid, an ammoniacal salt of it would be obtained in the quick process for the manufacture of vinegar, where the woody fibre undergoes a slow decay with the alcohol. As yet however no ammonia-formation has been observed.

The process of decay of organic substances which contain little or no nitrogen at the surface of our planet, is as old as the occurrence of living matter upon it; it is so general and everywhere perceptible that our atmosphere would soon be poisoned with ammonia, there being no such chemical attractions for *nitrogen* gas (as an element) as for oxygen, and its amount of nitrogen would certainly have decreased, if this most indifferent of all gaseous elements possessed the property of contributing as such to the formation of ammonia.

LIII. *On the Iodide of Mercury. By* H. F. Talbot, *Esq., F.R.S., &c.*

To the Editor of the Philosophical Magazine.

Dear Sir,

GIVE me leave to reply in a few words to some remarks inserted in the last Number of your Journal. I will not occupy much of your space in so doing, for this appears to me to be really a very simple case. It is a mere question of *dates*, and nothing else.

Finding a paper inserted in your Journal, describing chemical and optical phænomena which appeared to be almost exactly the same with what I had published six years previously, I took the liberty of calling your attention to it. At the same time I carefully discriminated, as being "a fact both new and important," the phænomenon described in the latter part of the paper, since it was different from anything which I had observed. I am at a loss to know why objection should be taken to such a communication as this. It was simply a claim

* *Journ. für Prakt. Chemie,* Bd. xxvii. S. 165.

of scientific justice, such as inevitably must frequently be made in these days of unexampled activity in experimental research; for no one can possibly be expected to read and remember all that is published in the journals of science at home and abroad. Mistakes are therefore continually occurring. Discoveries already made are again published as new. When this happens, no other course is open than to point out the error, and to correct it in as few words as possible. This is what I intended to do, and it was my only object in addressing to you my short letter on the subject. The credit that may be due to any scientific discovery, whatever it may be, ought assuredly to be awarded to the first discoverer, and that with all the care and correctness that is possible; and I am quite ready and willing to see this principle fairly and impartially applied in all cases—more particularly in the present one.

Mr. W. has satisfactorily shown that the very pretty phænomenon of the iodide of mercury was first observed by Mr. Hayes, an American chemist, who published it fourteen years ago in Silliman's Journal. By all means then in future let it be ascribed to Mr. Hayes. It is to be regretted that his claim was not sooner mentioned; but I suppose that no one was aware of it.

I have referred to the page indicated of the American journal, but I find nothing more there that has reference to this particular subject. Mr. Hayes does not appear to have noticed the definite form and rectilinear boundaries of the scarlet portions of the crystal; which fact adds something, I think, to the argument, that the change of colour is owing to molecular displacement, and not to the loss or gain of any substance whatever.

With respect to the very different phænomenon seen in the *iodide of lead*, I believe that if any chemist would take the trouble to ascertain exactly what passes during its rapid transformation, this could not fail to be a valuable contribution to science; for at present it remains one of the most enigmatic facts in crystallography.

London, March 11, 1843. H. F. Talbot.

LIV. *Notices respecting New Books.*

An Introduction to the Study of Chemical Philosophy, being a Preparatory View of the Forces which concur to the Production of Chemical Phænomena. By J. Frederic Daniell, For. Sec. R.S., Professor of Chemistry in King's College, London, &c. &c.

The author of this volume informs us in his preface to the first edition, that " the origin of his work was a desire to present to students of chemistry an elementary view of the discoveries of Dr.

Faraday in electrical science," and he justly remarks that "the successive memoirs of an experimental philosopher must necessarily be better adapted to the study of the proficient than to the instruction of the beginner, and that long periods of time often elapse before the facts which they contain would find their places in general systems."

To this we should be inclined to add, that the piece-meal mode in which this is commonly effected generally produces a patch-work of old and new matter, extremely unfavourable to the reception and spread of new and important opinions. Professor Daniell, however, having early perceived that chemical philosophy would date one of its most splendid epochs from the publication of "The Experimental Researches in Electricity," and that they not only extended but simplified the general theory of the subject, has composed his work with a single view to the *molecular induction* of statical electricity, and *the definite chemical action* of dynamical electricity; these principles he has gradually applied in explanation of the first elementary facts of the science with a unity of effect which cannot but be highly favourable to their general reception. The superiority of the new theory over the old is manifest from the first apparent repulsion of the leaves of an electroscope to the complicated phænomena of the Leyden jar and the Electrophorus, and the facility with which the complicated relations of voltaic circuits are explained upon the doctrine of current affinity is calculated to shake the confidence of the stoutest advocate of the contact hypothesis.

As a guide to the physical arrangement of the voltaic battery, Professor Daniell has likewise adopted Professor Ohm's view of electromotive forces and resistances, by the introduction of which from the beginning he has been enabled to give a popular illustration of his celebrated formula.

On a future occasion we shall probably make some extracts from the Professor's work which belong more strictly to the chemical portion of it; at present we content ourselves with having pointed out that portion of the work which is peculiar to it, and which consequently, we believe, is not to be found in any other similar work on chemistry.

LV. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 231.]

February 10, 1843.—*Extracts from the Report of the Council to the Twenty-third Annual General Meeting.*

THE Council regret that they have to lay before the meeting a list of a much greater number of deceased Members than on any former occasion: no less than eleven, during the past year, being removed by the hand of death; amongst whom are some of the earliest members of the Society. Although these sad events must always be a source of regret to the members, yet the Council trust

that it will act as a stimulus to those who survive, to repair the loss which has been thus occasioned, and lead to new efforts for the promotion of the objects which it has in view.

The Earl of Macclesfield was one of the early members of this Society, and continued his connexion with us till the time of his death. He was the great-grandson of the first Earl of Macclesfield, who was President of the Royal Society in 1752; and who had established an astronomical observatory, fitted up with excellent instruments, at Shirburn Castle, near Oxford, which Bradley frequently visited. The observations made at Shirburn are now in the Savilian Library at Oxford; and Bradley acknowledged his obligations to them in enabling him to complete his researches on nutation and refraction. The observatory does not now exist; but in the library, which is still in great preservation, and contains many valuable printed works, there is a large collection of original letters, from men of science, in the last century; amongst which the names of Oughtred, Flamsteed, the Gregorys, Barrow, Wallis, and Newton, frequently occcur. The late Earl of Macclesfield, whose decease we are now recording, permitted the late Professor Rigaud to make a selection of such letters as he thought might be most interesting to the public, which have since been printed at the University press of Oxford, and at the expense of the delegates, in two volumes. Amongst these documents is the first letter which Flamsteed wrote to the President of the Royal Society, on Nov. 24, 1669, and which was not known to be still in existence prior to this discovery. Only a portion of it was printed in the Philosophical Transactions; and in the present publication there are yet certain portions omitted, which are not now considered to be interesting.

Mr. Smeaton was a civil engineer, and was descended from a brother of his celebrated namesake: he had been but a short time a fellow of the Society when he was removed by death.

Mr. Thomas Tulley was the second son of Charles Tulley, the optician, in whose workshop he was brought up, and to whose business he succeeded, first in partnership with an elder brother, and afterwards alone, on the death of the latter.

Rear-admiral d'Urban has been dead some years, but the Council did not receive any news of his decease till very recently.

The Rev. Michael Ward had been a fellow of the Society for a long period. He was fond of astronomy, and possessed a small observatory.

Major-General Shrapnell was well known to military men as the inventor of the destructive shell which bears his name. He was for many years a fellow of the Society, though his period of active exertion was almost passed before the Society was established.

Mr. James Moore French, chronometer-maker, at the Royal Exchange, was a zealous and successful artist, and on several occasions gained the prize given to the best of the chronometers which were tried at Greenwich.

Captain William Tucker, R.N. was introduced to the Society by his uncle, the late Mr. Frend, of whom an account appears in the last annual Report. He perished in November last, in the 48th year of his age, on the wreck of the unfortunate East Indiaman Reliance, which was lost off Boulogne. The circumstances of his death and the devotion of his last moments, as narrated by survivors, to the performance of an act of humanity, created the strongest public sympathy. He had been almost all his life in active service, and particularly in cruising against the slave-trade, in which he had been remarkably successful; and he gained his commander's commission by a daring and prosperous attack upon a slaver of twice his force. He had been, previously to his death, in command of the Iris frigate, and senior officer on the Cape Coast station, and the failure of his health, which obliged him to return to England in a merchant-vessel, led to his unfortunate catastrophe.

Commander Michael Atwell Slater, R.N., was an officer who had gained distinguished reputation in the scientific branch of his profession; and was well known to many members of this Society for his zeal in the extensive surveys in which he was occasionally engaged. It was in one of these useful labours that he was unfortunately cut off in the prime of life, on the 2nd of February in last year, by falling into the sea over the cliff called Holburn Head, on the eastern extremity of Scotland.

Mr. Innes of Aberdeen was well known to astronomers as a zealous calculator of eclipses, occultations, and tides; which occupied a considerable portion of his time. He was brought up as a watch and clock-maker: and although his professional gains were but small, yet by living very economically, he was enabled to collect together a valuable collection of books, which he has left behind him. He was a man of very mild temper and unassuming manners; and, after a very slow decay of health, died on the 22nd of May, 1842.

The Council feel sure of the approbation of the Society at large in the award of a Gold Medal to Mr. Baily, for his persevering and skilful management of, and complete success in, the repetition of the Cavendish experiment. The President has undertaken to explain in detail the grounds of this resolution, and to state to the meeting the more than usual obligation under which the Society has been laid by Mr. Baily's patient and sagacious proceedings. The publication of the 14th volume of the Memoirs, which is wholly devoted to an account of this experiment, renders any description, even of its general features, unnecessary in this place; but the Council cannot here refuse themselves the pleasure of recording their opinion, that in no instance whatever, since the foundation of the Society, has its medal been more worthily won, whether the result be looked at with respect to the skill and industry by which it was attained, or to the complete sufficiency of the Memoir in which it is promulgated.

While on this subject, it may further be stated that the 13th volume of the Memoirs, referred to in the last annual report as about to be presented to the Society by Mr. Baily, and containing the catalogues of Ptolemy, Ulugh Beigh, Tycho Brahé, Halley, and Hevelius, is now nearly completed, and nothing but the attention requisite for the Cavendish experiment has prevented it from being actually ready. Thus the Society receives, in the course of one year, two of the most valuable volumes of its Memoirs, both from the labour, and one at the expense of the same Fellow, and that Fellow the one of all to whom the Society is most indebted, independently of these rich contributions.

In the course of the last year, the question has been started, whether it would not be advisable to alter the numerical typography now in use, and to return to the old method of forming the Arabic figures, in the manner still usually practised in handwriting. A committee appointed to consider this subject reported unanimously in favour of the alteration, and the Council have accordingly given directions that it shall be carried into effect in all the future publications of the Society. The printers have met the proposition with a readiness which deserves the thanks of the Society, the change involving, as it does, some trouble and expense. Fortunately, however, it has been found that though the old type has been almost entirely disused for many years, the punches necessary to re-cast it are still, of every size which the Society wants, in the hands of the type-founders. The Council strongly recommend the alteration to the fellows in their own private publications, as they are sure that the form now in use bears no comparison, as to distinctness and legibility, with that which it is proposed to restore.

During the past year, the trustees of the Radcliffe Observatory at Oxford have, for the first time, published the observations made at that establishment, in an octavo volume, containing the observations made in the year 1840. The director of that observatory, Mr. Johnson, is one of our most active members, and well known to us as the author of the excellent catalogue of southern stars, printed at the expense of the East India Company, and rewarded by this Society with its Gold Medal in the year 1835. Mr. Johnson, conceiving that it would be desirable to confine his attention principally to a selected class of observations, determined to re-observe those stars in Groombridge's catalogue that are situate to the north of the zenith of his place. The volume, here mentioned, contains the first attempt of this kind: and in it we see the same marks of minute accuracy and scrupulous integrity that were so evident in his former publication. The Council trust that the publication will be continued in like manner from year to year, as it is only in this way that the progress of discovery can be rendered of essential and permanent advantage.

It has been mentioned at the preceding anniversaries of this Society that the British Association had appropriated funds for three very useful and important catalogues, which the Council are

now happy to state have been completed. The first is an extension of the catalogue of this Society, published in the second volume of its Memoirs, with certain additional columns that will render it of greater value to the practical astronomer. The British Association have granted the funds requisite for the publication of this work, which will soon be sent to the press. The next two catalogues contain the reduction of the stars observed by Lacaille at the Cape of Good Hope, and of those observed by Lalande at the *Ecole Militaire* at Paris. These catalogues are finished, and the British Association propose to apply to government for a grant of the requisite funds for printing them.

The Council, while reviewing the subjects connected with astronomy which have been brought before the Society since the last anniversary meeting, beg particularly to call the attention of the meeting to the labours of Professor Henderson and of M. Hansen. It will be remembered that in the President's address, on delivering the Gold Medal to M. Bessel for his researches on the parallax of the double star 61 Cygni, honourable mention was made of the labours of Professor Henderson in a similar inquiry with respect to α Centauri, founded on the reduction of his own observations made with the mural circle at the Cape of Good Hope. This indefatigable astronomer has within the present year presented us with the result of a series of observations made at his request by Mr. Maclear expressly for the purpose; and which, extending as they do considerably beyond a year, or the time during which the parallax goes through all its changes, and averaging from eight to ten observations of the double altitude of each star in every month, will at least afford good ground for determining whether the problem of the parallax of this remarkable star is likely to be solved by meridian observations. But without entering into the question of the evidence offered by the observations, our thanks are certainly due to the untiring zeal of Professor Henderson in prosecuting this most important but too much neglected branch of astronomy, and in this expression the Council feel sure that the meeting will join.

The meeting will scarcely need to be reminded of the discovery of M. Hansen, to which allusion has been made, his letter having been so recently read before the Society. The want of some general method of expressing the perturbations of a body moving in an ellipse, whose eccentricity and whose inclination to the ecliptic are not small, has been, as it were, the opprobrium of modern physical astronomy. The method which has hitherto been employed of dividing the orbit into several portions, calculating the differentials of the perturbations for the points of the orbit thus decided on, and then integrating them by mechanical quadratures, seems scarcely worthy of the present state of analytical science; and has put the patience of astronomers to the severest trials as often as the return of an interesting comet, such as Halley's or Encke's, has made necessary the rigorous computation of its disturbances by the larger planets. Still, such has been

the difficulty of the problem, that up to the present time no person since Lagrange seems to have suggested or hoped for any means of removing it. M. Hansen has laid before us the result of his first trial in the case of the comet of Encke, and the comparison of his computed perturbations with those made by Encke by mechanical quadratures, proves the accuracy of the method as well as the easy application of it. We may be allowed to express a hope that we shall be soon in possession of the method itself, which we are thus far entitled to regard as a most brilliant conquest over one of the residual difficulties of physical astronomy. The Council cannot refrain from congratulating the meeting on the above proofs that the science which we especially cultivate is still advancing; that each year adds something to our stock of previous knowledge of the constitution of the universe, and that, too, of an importance that marks the zeal and the talent with which, both at home and abroad, preparations are making for the complete solution of the few most interesting problems which yet remain to us.

The Council have great pleasure in drawing the attention of the meeting to the very valuable present which the Society has recently received from Admiral Greig, one of our members, and a distinguished officer in the Russian navy. This consists of an altitude and azimuth instrument, by M. Reichenbach* of Munich. The diameter of the azimuth circle is 15, and of the altitude circle 12 inches. The divisions, which are upon silver, read to $4''$ of space by 4 verniers upon each circle. The instrument is one of the kind which admits of repetition both in the horizontal and vertical planes, and is furnished with two telescopes; the principal one resting in Ys attached to the azimuth index, and the other placed below the azimuth circle, according to the ordinary arrangement. But a peculiarity deserving especial notice is the manner in which the usual difficulty of observing near the zenith is obviated. A diagonal reflector, in this case a prism, directs the rays through one of the pivots of the transit axis, in which the diaphragm and eye-piece are consequently placed, and thus the observer remains in an easy, unaltered position, whatever may be the altitude of the object observed. It is almost superfluous to add, that the graduation, axes, tangent screws, and other delicate parts of this instrument, exhibit all the proofs of care and skill for which the maker was so long celebrated.

The Council feel confident that the meeting will unite with them in a warm expression of their thanks to Admiral Greig, not only for his munificent present, but also for the proof he has thus given, that, though at a distance, he still looks upon us and our proceedings with that interest and regard which are doubly grateful as coming from an absent friend.

* In the Notice for January 1843, Article III., [Phil. Mag. pres. vol. p. 231.] the instrument is erroneously stated to have been made by M. Ertel.

The President (Lord Wrottesley) then addressed the Meeting on the subject of the award of the Medal, as follows:—

Gentlemen,—I have now the gratifying duty of stating to this meeting the grounds on which the Council have thought it right to award a Gold Medal to Mr. Francis Baily for his experiments to determine the mean density of the earth, in repetition of what is generally termed the *Cavendish Experiment**. In the performance of this duty, I am necessarily required to offer a few remarks on the nature and utility of the end sought, on the previous attempts which had been made to gain it, and on the manner in which the one now before us was conducted by the distinguished friend of the Society to whom we are this day to offer our highest token of acknowledgement.

The labours of the astronomer are directed not only to the accurate determination of the motions of the heavenly bodies, but also to that of their constitution and organization. If the papers in our Memoirs and those of other kindred societies seem to dwell much upon the former division of the subject, and little upon the latter, it is not of preference, but of necessity. Our means of determining satisfactorily anything which relates to the constitution, even of the bodies of our own system, are few and limited; while those which apply to the prediction of their relative motions constitute the most perfect body of science which exists. But all which is known, certainly or conjecturally, of the interior arrangements of the heavenly bodies, is of the highest interest, and most especially when its action upon the minds of men is considered. Those who have heard the son and successor of the patriarch of this branch of astronomy, on occasions similar to the present one, deliver his views upon the general constitution of planetary systems, cannot but have felt that the subject which could inspire such thoughts must, were it for that reason only, be among the noblest to which human energy can be directed.

The masses of the planetary bodies are important data of the Newtonian theory of gravitation, but only in a relative sense. If, at any given instant, each one of the innumerable particles of which the universe is composed were to acquire a doubly attractive power, and, at the same moment, a double resistance to the alteration of its state, the effect of the change would be unseen and unfelt, so far as the motions of the system are concerned. Nothing, therefore, is needed, in this last point of view, except a knowledge of the relative quantities of matter contained in the different planets.

There is something vague at the outset in the term *mass* or *quantity of matter*. With this the calculator of the planatary motions has nothing to do: the term *mass* is to him merely a convenient name for the numerator of the fraction which expresses the attractive force of that planet; and what number stands in any one numerator is of no consequence whatever, provided that all

* See Phil. Mag. S. 3. vol. xxi. p. 111.

the other numerators are in their proper proportion to that one. Hence the mass of the earth, for instance, may be called unity, and the results of observation may be made to give the relative masses of other bodies.

What, then, is the mechanical signification of this numerator? The answer to this question belongs to the physical astronomer, properly so called.

Our first idea of mass is derived from bodies at the surface of the earth; under the same bulk some are lighter, others heavier. Two explanations present themselves: either the heavier body is composed of matter of the same kind as the lighter, but in a state of greater density, or more closely packed; or else the heavier body is composed of particles intrinsically heavier, that is, more powerfully acted on by the earth than those of the lighter. Which of these two explanations is to be received as the true one, more concerns the chemist than the astronomer, but the language of the former explanation is always used in our science. If we have reason to know that water compressed into one-twentieth of its bulk would produce all the mechanical effects of gold, then the latter is, and must be, in mechanics, considered as containing twenty times the quantity of matter of the former, under a given volume.

We are in the habit of referring solid bodies to water, in estimating their quantities of matter, and the specific gravities of the several kinds of matter, or the ratios of the weights of given bulks to the weight of the same bulk of water, have been carefully determined. Now among the questions of primary interest to the physical astronomer comes the following:—What is the quantity of matter in the whole earth, from which that in the several other planets is so easily determined? If the earth were a ball of water, what alteration would need to be made in the numerator of the fraction which expresses its attractive power? Both these questions are answered together; and it is only within the last seventy years that any attempt at an answer has been made on rational grounds.

If we knew the materials of which the earth is composed, it would be a mere matter of calculation to answer the preceding questions. But, ignorant as we are, *à priori*, even as to the fact whether there is an interior to the earth at all, that is, whether it is a solid globe or only a hollow shell, we cannot possibly be in possession of the means of forming so much as a guess at the character of the answer. There is but one mode of proceeding, and that was, no doubt, suggested by the processes and results of the theory of gravitation. Since a comparison of the actions of two planets upon a third can be made to give the ratio of the masses of those planets, it is obvious that if we can compare the effect of the whole earth with the effect of any known part of it, we may deduce a comparison of the mass of the whole earth with the mass of that part of it. A mountain in one case, a large leaden ball in another, have been weighed against the whole earth, by com-

parison of their effects upon a pendulum: the nearness of the smaller mass making it produce a sensible effect as compared with that of the larger; for by the known laws of attraction the whole earth must be considered as collected at its centre.

Before giving any account of the different modes pursued, it may be permitted to pause a moment, and to consider the bearings of the result. Independently of the satisfaction which the mind receives from the conversion of relative into absolute knowledge, independently of the stimulus given to the advance of inquiry by the acquisition of such a resting point as the determination before us, and of the value which it adds to the theory of gravitation as an instructor of the world at large in the laws of nature, by enabling the teacher to present what would have been a mathematical conception in a more physical and tangible form—there are consequences of no mean importance which spring from the comparison of the whole earth with one of its definite parts.

I do not undervalue considerations which present themselves at every step which is made, at every height which is scaled, when I confine myself on this occasion, and before this Society, to those only which particularly concern just knowledge of the foundations of astronomy, and fitting preparation for its advance among the physical sciences.

When the *Principia* was published, one of the first who gave an unqualified adhesion to the general views of Newton was the justly celebrated Huyghens*. But there was one point which he could not bring himself to admit; it was the universal attraction of every particle upon every other. He could not feel certain that because the attraction of the whole of one planet upon the whole of every other was established, it followed that the parts of each attracted the parts of all the rest and of each other. Newton did not, and could not, make this important conclusion a fundamental experimental fact, though he was able to advance almost an overpowering presumption in its favour: and it is easy to see how the student of Descartes, even when he had been brought to admit the attraction of planet upon planet, might have suspended his opinion as to the action of part upon part, until the cause of the phænomenon, an inquiry much agitated in those days, was settled. But though the strong balance of probabilities in favour of Newton's opinion gradually gained for it a universal reception, there was no ocular and crucial evidence for the physical fact until the experiments were made which I shall presently describe; and of those experiments, the one of Cavendish, which Mr. Baily has repeated, was by far the most convincing. Seeing is believing: and no one acquainted with the apparatus, and actually noting the visible effect of the approach of a leaden ball upon the oscillations of a torsion pendulum, after the nicest precautions had been taken to remove any possible effect of currents, magnetism, electricity and heat, could doubt the presence of a totally distinct agent, pro-

* *Hugenii Opera Reliqua*, vol. i. p. 116.

ducing the effect of attraction, and transmitting its influence through all the screens which were employed to exclude disturbing causes, with as much ease as through space occupied only by air.

It had been previously shown by Laplace, that the mean density of the earth must exceed that of the sea, in order to preserve the stability of the oscillations of the latter: and subsequently Poisson, in a paper contained in the seventh volume of the Memoirs of the Institute, concludes (taking Cavendish's value of the mean density as one of the data, and applying the forces with which the sun and moon act in causing precession, in a manner which is perfectly just, though open to a small amount of objection against the numerical accuracy of the remaining data) not only that there must be an increase of density from the circumference towards the centre, but that, even on the supposition of an homogeneous nucleus, the variable strata which enwrap this nucleus must extend to a depth of at least one-fourth of the radius. The physical astronomer, using that power of judging which the study of past discovery has made habitual to him, has long considered it as all but proved that our terrestrial globe is not only solid to the centre, or at least having its interior strata composed of fluid with the density of a solid, but also that the density of successive strata is gradually increasing from the surface to the centre itself. But those who have not studied physics, and even experimental philosophers of note who have not paid particular attention to the theory of gravitation, have frequently taken the hypothesis of a hollow globe as being that which has the highest probability in its favour. The late Professor Leslie, for example, takes the hollowness of our earth for granted, and proceeds to reason upon the manner in which the part of the interior which is destitute of solid matter is filled. It is his opinion that it is a sure result of induction*, that "the great central concavity is not that dark and dreary abyss which the fancy of poets had pictured. On the contrary, this spacious internal vault must contain the purest ethereal essence, *Light* in its most concentrated state, shining with intense refulgence and overpowering splendour." It is not my intention to discuss the grounds upon which, taking comparative hollowness for granted, the void is to be supposed filled with light or any other æther, but only to observe that the experiment before us strikes most completely at the preliminary assumption. Maskelyne made the earth to be a ball five times as dense as the same bulk of water; Cavendish, five times and a half; but Mr. Baily, whose experiments are far more numerous and well supported, five times and two-thirds. The solid rocks with which the geologist professes that his knowledge of the interior strata of our earth terminates are only between three and four times as heavy as water: barytes itself, which takes its name from the great density of its compounds, has one of the heaviest of those compounds, the sulphate, under four times and a half its weight of water. If we were to ask what substance must be chosen, so that

* Elements of Natural Philosophy, vol. i. p. 453.

a globe uniformly consisting of that substance might take the place of our earth in the planetary system, retaining its size, the answer would be that nothing under the ores of silver or the lighter ores of lead would serve the purpose. The increase of density, then, from the surface towards the centre, is fully confirmed by this experiment: a highly probable result of the theory of gravitation is made as sure as any result can be. But while we thus admire the manner in which one inquiry is made to confirm the consequences of another, we must not forget that it is no small part of our province to apply the conclusions of sound knowledge to the destruction of those remains of the age of speculation which yet linger about the porch of inductive philosophy; and at which those who have safely gained the inner court will feel little temptation to laugh, when they remember how many may be, and probably are, led finally away by the delusions which lie in wait at the entrance. Among these the assumption that the earth is a mere hollow shell has held a conspicuous place: let us take this assumption, and grant that the depth of the solid matter is even one-eighth of the whole radius, which is more than many speculators would admit. To make such a supposition consistent with the result of the experiment before us, it must be inferred that the actual matter of the shell considerably exceeds mercury in mean density, and is all but equal to hammered gold. Let such a result be established, if it be possible; but in the meantime, and until presumption can be shown in its favour, let it be the office of the experiment before us to check the wildness of mere hypothesis.

The French academicians, in measuring their South American degree, were the first who found sufficient local attraction in a mountain adjacent to their observations (Chimboraço) to give any hope of making that phænomenon useful in future inquiry. Maskelyne, in 1772, suggested the employment of astronomical observation in the neighbourhood of a mountain, for the determination of the earth's mean density. Schehallien was chosen for the purpose by a committee of the Royal Society, and the result of this celebrated experiment was announced in 1775. The description of this purely statical experiment is easier than in that of Cavendish: the position of a plumb-line, in a state of deviation from the vertical of the place, on account of the attraction of a mountain, is first to be accurately determined by making that plumb-line the regulator of an instrument for measuring zenith distances, and comparing the zenith distances thus obtained with those determined in other places of known differences of latitude. The weight of the plumb-line is then acted on by three forces of known direction, one of them being of known magnitude. The remaining forces (one of which is the attraction of the mountain) can then be determined: so that if the distance of the mountain be known, its mass can be compared with that of the whole earth. The next step, and the most difficult one, is to compare the mean density of the mountain with that of water, which requires an accurate knowledge of its

material and size. Maskelyne's rough computation, made with such knowledge as he had of the composition of the mountain, gave the mean density of the earth nine-fifths of that of the mountain, or from four to five times that of water; Hutton's subsequent and laborious investigation made four and a half for the ratio; but Playfair's examination of the strata of the mountain led to the inference that the result of the experiment could only be considered as placing the same ratio between four and six-tenths and four and nine-tenths. It is worth noting that Newton (*Principia*, book iii. prop. 10) had ventured a conjecture, which, as happens so frequently with him, turns out to be true. He thinks that the mean density of the earth is *between five and six times that of water.*

The method first proposed by Michell, and adopted and executed by Cavendish, was wholly independent both of astronomical data and of the uncertainty of the material of comparison. A horizontal pendulum, suspended by a wire, the torsion of which caused it to make slight oscillations about a position of equilibrium, was substituted for the gravitation pendulum, or plumb-line; while a massive leaden ball took the place of the mountain. If the torsion pendulum had been as secure of its position of equilibrium as that of gravitation, the experiment would have been almost as identical in its details with that of Maskelyne as it is in its principle. The time of an oscillation would first have led to the settlement of the amount of torsion force in any given position of the pendulum: it is well known that nothing but the time of oscillation is wanted to give the means of determining the quantity of restitutive force which acts on the pendulum at any degree of departure from equilibrium. The degree of departure caused by submitting the ball at the end of the torsion pendulum to the lateral influence of the large mass would then have given the means of calculating the attraction of that mass, just as the amount of deviation of the plumb-line gave that of the mountain in Maskelyne's experiment. Small as may be the leaden mass compared with the mountain, it was so much better known as to make the risk of error materially less; to which must be added, that it was submitted to an instrument of very much greater delicacy than the ordinary plumb-line.

The torsion pendulum is, in fact, possessed of such an extreme susceptibility, that every detail of the experiment requires adaptation, to an extent which would make a superficial inquirer wonder how it could be in any way compared with that of the plumb-line and mountain. There is no position of equilibrium—the instrument is never at rest. The effect of presenting the large leaden ball is not to draw the torsion-rod from one position into another, but to change its motion from an oscillation of one extent about one point of rest to one of another extent about another point of rest; and a careful and peculiar mode (into which I cannot here enter) must be practised of determining this point of rest, both before and after the pendulum is placed under the influence of the

leaden mass. This point of rest, so called, is not at rest, and resembles one of the elements of an elliptic orbit, which would remain constant if there were no foreign disturbance, but is perpetually changing from the action of other planets. And so fast do the indications of the pendulum vary, that is, so rapidly does it change the character of its oscillation, even where no visible cause is at work, that it cannot be permitted to take a long series of observations to determine either the point of rest which precedes the change of position of the attracting mass, or that which immediately follows. For the instrument is always in a state of change, in a manner which would lead to the supposition that the torsion of the suspending wire is either visibly influenced by invisible changes of temperature, &c., or that it is acted upon by some agent of a character wholly unknown.

From seventeen experiments, Cavendish, in 1797, deduced 5·45 for the mean density of the earth; and nothing further was done in this species of experiment until 1836, when it was repeated by M. Reich of Freiberg, who followed Cavendish's plan in every particular, except in having one leaden mass instead of two. From fifty-seven experiments, M. Reich deduced 5·44 for the mean density of the earth,—a result almost identical with that of Cavendish.

In the year 1835, the Council of this Society appointed a Committee to consider of the best mode of procuring a repetition of the experiment. After some delay, occasioned by the difficulty of procuring funds, and choosing a site, the construction of the necessary apparatus was commenced at the end of 1837. On a representation made by the Astronomer Royal, the Government, in the year just named, granted 500*l.* for the purpose; and Mr. Baily (to whom the conduct of the whole was intrusted) chose, after much deliberation, to make his own house the scene of operation. About 400*l.* has been expended in the actual experiment, and it is right here to acknowledge the liberality of the present Government, which has sanctioned the application of 100*l.* remaining out of the sum granted by the late Government, to the payment of part of the expense of printing the results.

I could hardly undertake to make a description of the apparatus intelligible in little time, or without diagrams; but fortunately this attempt is rendered unnecessary by the fact of an abstract of Mr. Baily's paper having been for some months in the hands of the Fellows of the Society. Keeping in view, then, the main point of the present address, I shall make some remarks upon the progress of the experiment, with reference to the justification of the decision of the Council, on which it will presently be my duty to present a gold medal to Mr. Baily.

And first, with regard to the responsibility which was imposed. It ought to be most distinctly understood, that the functions of the Committee appointed by the Council ceased as soon as the money was obtained for the expenses, and Mr. Baily's offer of

superintendence was accepted. Not one direction was given, not one single condition was imposed, not one syllable was put upon record by which failure, had it occurred, could have been thrown from the shoulders of one upon those of several. The suggestions which Mr. Baily acknowledges himself to have received from men of science, whether in or out of the Society, were submitted solely to his judgement, as much as if the experiment had been his own private undertaking. Consequently, it is just that it should be most explicitly acknowledged that all the merit of success lies where all the blame of failure would have fallen. But, while I say this, I must also at the same time observe that the disposition to ask advice, and to try advice, which has characterized Mr. Baily's progress throughout, has been in no small degree conducive to the distinguished character of the result. The Council, when they placed in his hands, and at his sole disposal, the public money with which they had been intrusted, were well aware that no pains would be spared to collect, to compare, and to choose among, the best opinions which could be obtained. And while on this subject, it is proper to acknowledge the obligations of the Society to Professor Forbes, for the suggestion which overcame the difficulty and nearly destroyed the anomalies of the torsion pendulum, and to the Astronomer Royal, for his paper on the mathematical theory of Cavendish's Experiment, which will appear as a part of Mr. Baily's Memoir.

In the next place, it is necessary, before proceeding to the business of the day, to separate most emphatically the conductor of the experiment from the able and energetic friend of the Society in other respects. If an endowment had been bequeathed for the purpose of enabling the Council to give a yearly medal to the Member who should have been most active in carrying on the ordinary and extraordinary business of the Society, who is there that hears me—I speak particularly to those who are, or have been, on the Council,—who could positively undertake to say that such a medal could, up to this time, have been gained by any one except Mr. Baily? No doubt we have many among us whose active services the Society must most gratefully acknowledge;—our records prove that fact: indeed I think I may venture to say that ours has been a fortunate one among societies in the amounts of service rendered by individuals. No doubt, again, that the honourable leisure gained by a long attention to business has placed in Mr. Baily's hands a power of serving the Society which most of the Fellows do not possess. But this does not alter the fact, that he has been, ever since its foundation, identified with its progress, and assisting its efforts, in a greater degree than any other individual member. I state this now, not to acknowledge services which are so perpetually before the minds of all who take an interest in us, but to request you, if you can, to forget them for a short time, and to look on the experiment before us as if it had been the work of a new man, hitherto unknown to the Society, and

resting his claim to our gratitude wholly and solely upon its conduct and its result. It is only thus that you can fairly affirm the verdict which the Council has already given, and the approval of which it so confidently expects at your hands; for you must remember, gentlemen, that no services whatever, except those expressed in the resolution awarding this medal, can count in the smallest degree as a justification of that resolution. I am the more desirous of impressing this upon you, when I consider that this is the first occasion on which a medal has been awarded for the manner of employing money intrusted by the public to our charge: so that in fact, this defence, if I may so call it, of the award, is more than the explanation of the Council to their constituents, or at least must become something more. When it has gained your approbation, it must go forth to the public, and to the Government, as the account which the Society renders of the funds which were placed in its hands, as the proof that those funds were worthily administered. I am not, of course, alluding to the mere vouchers of pecuniary integrity, to the proof that money asserted to have been spent upon apparatus was actually so expended, but to the justification of a yet higher character.

The actual observations, meaning those which are printed in the Memoir, are 2153 in number, varying from ten to thirty minutes each; so that I am under the mark considerably when I say that 600 hours were spent at the apparatus, in the mere act of watching the oscillations of the torsion rod. To this must be added nearly as many more in the series of experiments, of which the results were afterwards abandoned on account of the anomalies of the pendulum. Add to this the time expended in contrivance, in computation, and in deliberation, and it will appear that it is not often that any single inquiry has called forth so determined an exercise of industry and perseverance. The experiments were commenced in October 1838, and were continued until May 1842. With the exception of about a couple of months of interruption, caused by the severe accident which, as all present are aware, happened to Mr. Baily in the summer of 1841, there was a continued succession of trials. This long and patient investigation gives a peculiar value to the result: the effects of the several conditions of the atmosphere cannot but have been eliminated, since the *printed* experiments, or those which were made *after the anomalies of the pendulum were got rid of*, run over nearly a whole year.

I cannot but call your attention to the spirit in which every part of the investigation was conducted. When all possible precautions had been taken to secure the stability of the pendulum; when it had been ascertained that no degree of concussion, whether in the room which contained the apparatus, or in that immediately above, would produce any sensible effect upon it; when every pains had been taken to remove thermal, electric, or magnetic disturbance; and at the moment when it seemed next to certain that the result, whether that of Cavendish or another, was about to come out clearly,

easily, and honestly, it was found that the work was yet to begin; that, in fact, the torsion pendulum was subject to every species of anomalous motion. In vain were such new precautions taken as these appearances suggested, and for eighteen months, a period so easy to speak of, so difficult to employ in activity under discouragement, no clue appeared to the solution of phænomena which seemed to set all the known laws of mechanics at defiance. Nothing could be more even than the temperature, even at the times when the anomalies were greatest; so that the effects of heat seemed to be out of the question as an explanation. The torsion rod, secure in a wooden case with thick glass ends, seemed to bid defiance to all external cause of currents of air; and so completely was the pendulum isolated, even from the case itself, that the latter might be violently shaken without any perceptible effect upon the former. Cavendish and Reich had both observed corresponding anomalies, but both apparently considered that their effects would disappear in the mean of a large number of observations. Mr. Baily resolved not to quit the subject until the cause of the anomalies was detected, and upon no account whatsoever to present a result vitiated either by assumption of the nature of the difficulty, or by rejection of the observations which appeared most discordant. To this resolute and honest determination we owe it that the paper for which this medal is given today is hardly less valuable as a lesson upon the nature and use of the torsion pendulum in measuring small forces than as a determination of the mean density of the earth.

It was at last suggested, and, as I have before stated, by Professor Forbes, that possibly the radiation of heat from the large masses might, when they were brought up close to the torsion-box, or case of the pendulum, affect the inside of that case. It was already known that the evaporation of a few drops of spirits of wine sprinkled on the side of the case would produce a large and rapid effect on the pendulum. But between this frame and the large masses there had already been interposed a wooden screen, which, it was thought, would wholly prevent any effect of radiation. The suggestion above mentioned was accompanied by a recommendation that the outside of the case, and the masses themselves, should be gilt. Mr. Baily carried his precautions still further: the case was first wrapped in flannel over which a new case (gilt) was placed; the masses, the plank which carried them, and the interior of the frame-work which inclosed both torsion-box and masses, were covered with gilt paper; the leaden balls at the two extremities of the torsion pendulum were gilt and burnished; and the masses were made to stand, when at the nearest, a little further from the torsion-box. These precautions proved to be sufficient; the anomalies were substantially removed, showing themselves only now and then, and in smaller quantities. The lesson thus read to experimentalists on the effects of radiant heat will, it may be hoped, lead to further inquiries. Mere creation of difference of tempera-

ture was insufficient to point out the source of the anomaly: red-hot balls, lamps, or lumps of ice, placed near the torsion-box, failed to give any clue to the cause of the difficulty.

All the observations, good, bad, or indifferent, made since the removal of the effect of radiation, have been taken into account in the general mean; and they have all been printed at full length, in such a manner that any one can go through the whole of the calculation in every experiment, from the announcement of one of the raw data of observation at the telescope, to the deduction of the mean density of the earth. Thus, as no experiment was ever more honestly or diligently performed, so also none has ever been more completely or satisfactorily described.

Now as to the result. The mean of all the experiments gives 5·675 as the mean density of the earth, with a probable error of ·004. The balls used at the ends of the torsion pendulum have been lead of different sizes, brass, platina, zinc, glass, and ivory; and in some of the experiments, a torsion rod of brass, without any balls, has been used. Various modes of suspension have been employed, single copper wire, double iron, double brass, and double silk thread. The discordances which occur between the results of different balls, modes of suspension, or both, have every appearance of being the consequence of an insufficient knowledge of the torsion pendulum, and lend no countenance whatever to the suspicion that the attraction of matter upon matter varies in different substances. A moderate examination will show that there is no doubt that the discordances, being such as might have been looked for in any inquiry, must disappear in the mean of so large a number of observations.

We may, then, confidently assert that this important element of the physical part of astronomy is settled, within very narrow limits of error. But suppose, if possible, that a less degree of trust were to be accorded to the mere result; nay, go further, and imagine the theory of gravitation itself, the best demonstrated of all general laws, to be an unfounded delusion. Perhaps we are then, on such a supposition, to give a still higher degree of praise to the manner in which this inquiry has been conducted. All the experiments are published, and all the experiments are *facts*. I have no doubt, and those who hear me have no doubt, that attraction is as real an existence in physics as it is an explanatory hypothesis in mathematics; and experiments of the nature of that before us seem to me, as to you, to put this beyond doubt, both in the hands of Maskelyne, Cavendish, Reich, and Baily. But if we be wrong, how shall we ever be brought to know our error? How, except by experiments conducted with that firm honesty of purpose, and true absence of all bias, which has characterized those described by me today. I repeat that these experiments are facts, facts which are and will be true, facts which are the result of an inquiry in which the Newtonian doctrine was fairly thrown into the scale, and weighed by Nature's own weights. The confirmation of the general truth of Cavendish's

mean density of the earth is the *numerical* result: but one more assurance that a century of patient and truth-loving inquiry will not fail of its reward, is the *moral* result.

It generally happens that in the award of our honorary distinctions, our immediate interest is limited to that which we must always take in an extension of our science, whether of principle, of process, or of fact; from what country or quarter soever it may come. We always acknowledge an addition to astronomy as a claim upon our gratitude: how particularly then is it incumbent upon us in the present instance, when the character of the Society depended upon the fulfilment of the pledges under which the means of making the experiment were obtained. Had that experiment failed, had it shown that Cavendish had deceived himself when he thought of obtaining the earth's mean density by his now established mode, there might indeed have been regret, but there would have been no shame. In any human undertaking no censure need be justly feared, when it can be shown that extreme diligence, unshrinking honesty long| deliberation, and the most candid research after, and use of, the suggestions and advice of others, have marked its progress from the beginning to the end. But when to all this we have a right to add success—when we can feel that we now present to the philosophical world a result on which they may confidently rely, knowing that the history of the experiment bears evidence which cannot be mistaken of its intrinsic value—we must consider this medal a token of what I may venture to call the *personal* gratitude of the Society towards one of its body who feared neither responsibility nor toil in its cause.

When, at some future time, those who are to profit by the labours of our day shall, with improved instruments and extended knowledge, once more repeat the interesting experiment to which these remarks refer, they will find in the records of this attempt proofs of its honesty which are now hidden from us by those very instrumental deficiencies and theoretical imperfections, the removal of which will be the signal to renew the process. And, in like manner as we now render due honour to Cavendish, not only for the first actual performance of the experiment itself, but because, with comparatively rude apparatus and few trials, he came so near the truth of which he was in search, so will they remember to celebrate the patience, the integrity and the sagacity of the philosopher who made the next step, and who showed them the path of amelioration. I cannot better express my strong feeling for the honour and welfare of this Society, than by claiming your response to an earnest wish and desire that, when that time shall come, our Fellows may be among the foremost promoters of the revival of this experiment, and that they may find among themselves one to whom they dare intrust the sole superintendence, and who will justify their confidence as well as Mr. Baily has done that of your Council on the present occasion.

The President then, addressing Mr. Baily, continued as follows:—

Mr. Baily,—I present you with this medal in the name of the Society for which you have done so much, as the highest testimony which they have power to bear to the splendid service which you have rendered to the science of Astronomy. I thank you in their name for the care which you have taken of the honour of the Society, and for the augmentation which it has received from your labours. And to our sincere congratulations upon the providential escape which you experienced, during the prosecution of your inquiry, from a sudden and violent death, I add the expression of our earnest hope that you may long be spared to continue your services in the advancement of knowledge, and to enjoy that well-earned fame which will wait upon your life and your memory.

The following Fellows were then elected as Council for the ensuing year:—

President: Francis Baily, Esq., F.R.S.—*Vice-Presidents*: George Biddell Airy, Esq., M.A. F.R.S., *Astronomer Royal*; Augustus De Morgan, Esq.; Rev. George Fisher, M.A. F.R.S.; Lord Wrottesley, M.A. F.R.S.—*Treasurer*: George Bishop, Esq. —*Secretaries*: Thomas Galloway, Esq., M.A. F.R.S.; Rev. Robert Main, M.A.—*Foreign Secretary*: Captain W. H. Smyth, R.N. K.S.F. D.C.L. F.R.S.—*Council*: Samuel H. Christie, Esq., M.A. F.R.S.; Rev. W. Rutter Dawes; Thomas Jones, Esq., F.R.S.; John Lee, Esq., LL.D. F.R.S.; Captain W. Ramsay, R.N.; Edward Riddle, Esq.; Richard W. Rothman, Esq.; Rev. Richard Sheepshanks, M.A. F.R.S.; Lieut. William S. Stratford, R.N. F.R.S.; Charles B. Vignoles, Esq.

CHEMICAL SOCIETY.

[Continued from vol. xxi. p. 389.]

November 1, 1842.—Mr. Warington presented part of a cast-iron grating which had been subjected to the occasional action of slightly acid liquids for several years, and which exhibited the partial removal of the metal, while the residual graphite retained the original form.

The following communications were then read:—

51. "On Heat of Combinations," Part I., by Thomas Graham, Esq., F.R.S. (This paper will be inserted in an early Number of the Philosophical Magazine.)

52. "On Pyrogallic Acid," by John Stenhouse, Ph.D. (Inserted at p. 279. of the present Number.)

53. "On the Analysis of Organic Substances containing Nitrogen," by George Fownes, Ph.D.

The circumstance which led to the present note on the analysis of azotized organic bodies, was an attack lately made by M. Reiset on the new method of determining the nitrogen in such cases, put

into practice with great apparent success by MM. Will and Varrentrapp of Giessen.

After drawing a favourable contrast between the new method and those previously in use when the proportion of nitrogen to be determined is small, the author proceeds to inquire into the validity of the objections before alluded to. It is stated by M. Reiset that when sugar is burned with the usual mixture of hydrate of soda and lime in fine powder, and the gases evolved conducted into hydrochloric acid, an addition of pure chloride of platinum and evaporation to dryness gives rise to a quantity of the double chloride of platinum and ammonium, indicating in some experiments 1 to 1·5 per cent. of nitrogen in the body analysed; and as this was considered too great to be attributed to accidental impurity, it was ascribed to the absorption of the nitrogen of the air contained in the tube by the mixture of carbonaceous matter and alkali, and the subsequent conversion of the cyanide so formed into ammonia; and this idea was strengthened by repeating the experiment with the tube filled with hydrogen instead of air, when the production of ammonia was found to be lessened. It became important to know how this very serious objection could be disposed of.

On repeating the experiment, it was found that when the finest white sugar-candy was thus burned, a certain quantity of the yellow platinum salt always remained upon the filter after washing with the mixture of alcohol and æther, but this quantity, instead of indicating 1 per cent. or more of nitrogen in the sugar, gave in three experiments only ·06 per cent., a quantity attributable to impurity.

Tartaric acid and charcoal made from white sugar gave similar results, the ammonia amounting to a mere trace, doubtless due to foreign admixture.

It is difficult from such experiments to avoid drawing the conclusion that the appearance of the nitrogen is in all such cases due to accidental impurity in the body burned, and not to any direct or indirect formation of ammonia from the nitrogen of the air.

To those practising the new method under discussion the following observation may be useful:—in mixing the organic matter with the alkali in a smooth porcelain mortar some inconvenience is experienced in the obstinate adhesion of some of the mixture to the bottom of the mortar and also to the pestle, and which is often with difficulty removed by triturating two or three small successive portions of dry soda-mixture; the powder is too soft to cleanse perfectly the mortar, and a little left behind would necessarily occasion loss in the ultimate result. By the use of a few grains of finely powdered glass this inconvenience is obviated; the glass is rubbed for a few seconds in the mortar, which it cleanses in the most complete manner, and can then be transferred to the rest of the mixture in the tube, where its presence can occasion no injury whatever.

As additional testimony to the value of the new method, Dr. Fownes subjoins the results of a set of experiments made by himself with a view of testing the process before venturing to employ it upon bodies of yet unknown composition:—

Uric Aid.

	1.	2.	3*.	4*.
Substance	4·99	5·14	5·21	5·45
Platinum salt, with filter......	29·14	30·2	30·53	31·62
Filter	3·13	3·29	3·28	3·12
	26·01	26·91	27·25	28·5
Nitrogen	1·6506	1·7077	1·729	1·808
Per cent.	33·08	33·22	33·19	33·19

Theoretical per-centage.......... 33·36

Urea, with a little sugar.

Substance	4·17	
Platinum salt, with filter	34·	
Filter	3·35	30·65
Nitrogen	1·945	
Per cent.	46·64	

Theoretical quantity...... 46·78

Hippuric Acid.

	1.	2.
Substance	8·85	8·24
Platinum salt and filter ..	14·17	13·43
Filter	3·44—10·73	3·23—10·2
Nitrogen	·6809	·64729
Per cent.	7·7	7·85

By theory .... 7·82

Allantoin, with a little sugar.

	1.	2.
Substance...............	8·23	5·47
Platinum salt	45·61	30·47
Per-centage of nitrogen ..	35·17	35·35

Theoretical quantity...... 35·5

November 15.—The following communications were read :—

54. "On some Astringent Substances as Sources of Pyrogallic Acid," by John Stenhouse, Ph.D.

55. "On some new Cases of Galvanic Action, and on the Construction of a Battery without the use of oxidizable Metals," by Alexander R. Arrott, Esq. (Both these papers will shortly appear in the Philosophical Magazine.)

December 6.—Numerous specimens of rare chemical products were exhibited by Mr. Loyd Bullock.

The President (Prof. Graham) exhibited a stereotyped plate which had undergone a secondary crystallization by exposure to a damp atmosphere in contact with paper.

The following communications were read :—

56. Extract from a letter from Dr. Will, dated Giessen, November 10, 1842.

"I have repeated Reiset's experiments on the combustion of sub-

* Mixed with 4 grains of sugar.

stances free from nitrogen with caustic soda and lime. The result is, that his statements are incorrect. There is not a trace of ammonia formed if the alkaline mixture as well as the employed substances is quite pure, so that Reiset's observations are not at all an objection to our method for determining nitrogen. I believe Reiset's alkaline mixture contained nitre, or something else, otherwise he could not have obtained such results.

"From my experiments I was led also to repeat Faraday's investigations on the formation of ammonia, and believe I shall find the cause why he sometimes obtained ammonia and sometimes not, by heating non-nitrogenous organic substances, or zinc with hydrate of potash*."

57. "On Æthogen and the Æthonides," by William H. Balmain, Esq.

58. "Report of some Experiments with Saline Manures containing Nitrogen, conducted on the Manor Farm, Havering-atte-Bower, Essex," by M. W. F. Chatterly, Esq.

(These papers also will appear in an early number.)

December 20.—The following communications were read:—

59. On the Division by Three of the Equivalents of the Phosphorus Family of Elements," by Thomas Graham, Esq., F.R.S. (This paper will shortly be inserted in the Philosophical Magazine.)

60. "Remarks on the Determination of Nitrogen in Organic Analysis," by W. Francis, Esq. The presence of nitrogen in picrotoxine having been denied by all experimenters, the author was induced to repeat with great care the analysis of that substance, in the course of which researches abundant evidence of nitrogen was obtained. A few grains of pure picrotoxine, heated in a tube with a little of the mixture of lime and hydrate of soda, gave off vapours which quickly restored the blue colour to reddened litmus paper; the smell of ammonia was also quite distinct.

An analysis being made by the method of Messrs. Will and Varrentrapp, in order to determine the amount of nitrogen, distinct yellow crystals of the double chloride of platinum and ammonium were obtained, corresponding in one experiment to 1·3 per cent. of nitrogen, and in a second to 0·75 per cent.

Burned with oxide of copper, numbers representing the carbon and hydrogen came out closely corresponding to the results obtained by Regnault.

The observations of M. Reiset, in a late Number of the 'Annales de Chim. et de Phys.,' threw some doubts upon the value of the analytical method above mentioned, and the author was led in consequence to repeat the experiment on a specimen of carefully purified sugar: 1·649 sugar gave 0·048 of a brownish black substance on the filter, which calculated as the salt of ammonio-chloride of platinum gives 0·24 per cent. of nitrogen; on being burnt it left 0·035, which calculated as metallic platina = 0·30 per cent. nitrogen; 2·130 sugar

* Dr. Will's paper on these subjects will be found in the present Number of the Philosophical Magazine, p. 286.—Edit.

gave 0·053 of the black substance, and when burnt 0·31 of platina, affording in the one case 0·15, in the other 0·20 per cent. as nitrogen. The sugar, to ensure purity, had been crystallized twice out of an aqueous solution, again dissolved and thrown down by alcohol, collected and recrystallized out of water. A small quantity of it heated with some of the alkaline mixture in a test-tube, afforded vapours which *did not* affect the red colour of litmus paper. Frequently in analyses by this method, especially when the organic substance is very rich in carbon, fluid carburetted hydrogens distil over, which remain behind on evaporation, forming a black residue. This is not wholly dissolved on edulcorating with æther and alcohol, and goes to increase the weight of platinum salt, if there be any. The residue remaining on the filter after edulcoration with alcohol and æther in the second experiment, did not exhibit under the microscope the least trace of the yellow crystalline salt, but was of a blackish brown amorphous appearance. It appears, therefore, that the substance calculated above as ammonio-chloride of platinum was most probably platinum which had been reduced by these carburetted hydrogens during evaporation.

An analysis of oxamide by the new process gave an excellent result, the per-centage of nitrogen falling just below the theoretical quantity.

61. "On the Sugar of the Eucalyptus," by James F. W. Johnston, Esq., F.R.S., will shortly appear in our pages.

62. "On the probable existence of Nitrogen combined with Silicon in Soils and other Substances," by W. H. Balmain, Esq.

The stability of the compounds of boron and silicon with nitrogen, and the facility with which such compounds are produced when organic matter is strongly heated with a borate or silicate, seemed to render it probable that such bodies might occasionally exist in unexpected circumstances, as in soils or minerals for example, and experiments were made with a view of directly ascertaining whether this was the case.

Samples of several varieties of soil were boiled for some time with a mixture of dilute sulphuric and nitric acids, then washed and dried and subjected to the action of hydrate of potash at a high temperature; ammonia was in all cases abundantly disengaged, even after the purified soil had been heated to redness. In one instance the sample of soil was boiled with strong nitric acid as long as nitrous acid vapours were generated, then submitted to the action of dilute sulphuric acid, washed again, boiled with strong solution of caustic potash, washed, and then agitated with chlorine gas; yet on being heated with hydrate of potash it gave off ammonia abundantly.

It was inferred by the author, that the nitrogen ultimately found was in combination with silicon, and in that condition had resisted the action of the various agents employed for its removal.

January 3, 1843.—The following communications were read:—

63. "On Palladium, its Extraction and Alloys," by W. J. Cock, Esq.

64. "On the Formation of Fat in the Animal Body," by Justus Liebig, M.D.

January 10.—The following communication was read :—

65. "On the Formation of Milk in the Animal Economy," by Lyon Playfair, Ph.D.

February 7.—The following communications were read :—

66. "On a new Method of obtaining pure Silver in the Metallic State or in the Form of Oxide," by William Gregory, M.D., F.R.S.E. All these communications will shortly appear in the Philosophical Magazine.

67. "Some Experimental Observations on the formation of Prussian Blue upon the surface of Gravel, through the medium of Ferrocyanide of Calcium." By Robert Warington.

In a communication formerly made to the Society by Mr. Porrett on the above subject*, that gentleman considered the production of prussian blue to have arisen from some of the gas-lime employed to destroy the worms, &c., and placed under the fresh gravel, having been accidentally dropped on the surface, and that the peroxide of iron contained in the gravel had been deoxidized by some of the sulphur compounds of the gas-lime, giving rise to the formation of a combination of iron with cyanogen and calcium, and that this compound had been decomposed by the action of the carbonic acid of the atmosphere, or by the siliceous matter of the stone, thus causing the formation of prussian blue. An artificial ferrocyanide of calcium was formed by mixing hydrate of lime and prussian blue to the consistence of a cream; and this was placed in an open part of a garden, and numerous white-coated siliceous pebbles, selected from the red gravel of the neighbourhood of London, then partly immersed in the mixture, so that the upper surfaces might be exposed to the action of the atmosphere and moisture; in a few days the sides of the pebbles assumed the blue colour, which gradually spread itself to the summits, having the same bright tint as the pebbles presented to the Society by Mr. Porrett, proving therefore that the ferrocyanide had been drawn to the surface, either by that curious species of crystalline growth, if the expression may be allowed, which is exhibited by so many saline combinations during their crystallization, or by capillary attraction united with evaporation from the exposed parts of the pebbles, thus rendering it evident that the ferrocyanide might reach the summit of the gravel from below.

Other substances were then submitted to the same action, to decide the question as to the siliceous matter of the stones being in any way instrumental in the production of colour. White limestone pebbles, from the south coast of Devon, and baked pipe-clay, underwent the same changes, with the exception that the blue tint was not so bright and clear as was the case on the siliceous surface; but this is considered attributable more to the perfect whiteness of the siliceous coating, and the decidedly superficial film of prussian blue which was produced on it. Independent of this, the effect can only be attributable to the action of the carbonic acid gas present in the atmosphere slowly decomposing the ferrocyanide of calcium and generating the blue stain.

* See Proceedings of Chemical Society, p. 35, vol. i.

68. "On the Preparation of Malic Acid from Culinary Rhubarb," by Thomas Everitt, Esq., will shortly be transferred to our pages.

February 21.—The following communications were read:—

69. "A short Notice from Mr. Francis announcing the separation of Theine from the *Ilex Paraguayensis* or Paraguay Tea," by Dr. Stenhouse.

70. Extract from a letter from Professor Henry Croft, "On the Manufacture of Sugar from the *Zea Mays*."

Experiments have been made in the State of Indiana which seem fully to prove that the stalks of the maize may be employed advantageously for the manufacture of sugar. It is well known that the sugar-cane, as grown in Louisiana, does not produce above one-third as much saccharine matter as when raised in Cuba and other tropical situations. In Louisiana one acre yields from 900 to 1000 lbs. of sugar, and it appears that 1000 lbs. may be obtained from the stalks of the maize. The juice of the latter contains more than three times as much sugar as the juice of the beet-root, and five times that of the maple. By plucking off the ears of the maize as they begin to form, the saccharine matter of the stalk is greatly increased. The maize-stalks require less pressure, and the whole of the stalk can be used, afterwards affording a good fodder for cattle. The plant can be raised with the greatest ease in from seventy to ninety days, whereas the sugar-cane requires much care and attention, and does not arrive at maturity in less than eighteen months.

LVI. *Intelligence and Miscellaneous Articles.*

THE COMET.

We have been favoured by the Rev. W. R. Dawes, R.A.S., with the following particulars:

A large Comet has become visible in the evening, soon after sunset. It appears to have been first seen in this country on Thursday the 16th inst. by Mr. Shorts of Christchurch, Hants. But it had been observed on board the Tay, West Indian Mail Steamer, on her homeward voyage, as early as the 6th, and at Nice by Mr. Cooper, on the 12th. On the 14th Mr. C. detected the nucleus, which he found to be stellar, and equal to a star of the sixth magnitude; but its situation could not be correctly ascertained. At Paris it was first noticed on the 16th. On that day Mr. Cooper obtained, at Nice, a rough observation of the nucleus, from which he concluded that its right ascension was about 2 hours 30 minutes, and south declination 15 degrees. He also determined its *geocentric* motion to be *direct*, and *northward*.

Its appearance is remarkable; the tail being of great length, nearly uniform in brilliancy, and its lateral limits almost exactly parallel, while its breadth scarcely exceeds one degree and a half. On the 17th it was observed by Sir John Herschel as a vivid luminous streak, commencing close beneath the stars κ and λ *Leporis*, and thence stretching obliquely westward and downward between γ and δ *Eridani*, till the vapours of the horizon rendered it invisible. On Friday the 24th it was well seen. At about eight o'clock it was distinctly observed to extend from a little to the west of the star

δ *Monocerotis*, between *Rigel* and ι *Leporis*, nearly in the direction of ι *Eridani*: and Sir John Herschel obtained a view of the head, which he concluded to be near one of the stars of ρ *Eridani*; its appearance being that of a star of the fifth magnitude, but dim, and having no sharp nucleus. The star 63 *Eridani* was *in* the tail, a trifle north of its axis. No bifurcation could be perceived; the axis being throughout rather the brightest part. Its direction is very nearly parallel to the equator, though a slight curvature may be suspected, the convexity being northward. By comparing the observed place of the tail on the 24th with that noticed by Mr. Cooper on the 14th (when it passed over γ *Eridani*), it appears to have advanced northward about four degrees in the interval of ten days. This direction and quantity of motion was confirmed by an observation on the 25th, when 63 *Eridani* was found to be still *in* the tail, but near its *southern* border.

The almost constant presence of cloud or haze near the south-western horizon has greatly interfered with observations of the nucleus; but it is to be hoped that ere long it will be satisfactorily observed with the large fixed instruments at some of our principal observatories.

RESEARCHES ON THE FORMATION OF MOSER'S IMAGES.

Extract from a Letter from M. Fizeau to M. Arago. *Comptes Rendus*, Feb. 13, 1843, p. 397.

"In a letter which I had the honour of addressing to you, and which you were pleased to communicate to the Academy of Sciences on the 7th of November (see Scientific Memoirs, vol. iii. p. 488), I mentioned some experiments relative to the phænomena observed by M. Moser*, namely, the formation of the images that make their appearance on a polished surface when bodies are placed very close thereto. These experiments had led me to consider these novel facts, contrary to the opinion of Moser, as foreign to every species of radiation, and to ascribe them to the well proved existence of fatty and volatile matters which soil the surface of most bodies.

"Not having completed the inquiry which I shall have the honour to present to the Academy on this subject, I am desirous of acquainting you with the principal facts on which the explanation which I propose is based.

"1. The property of forming images on a polished surface is not permanent in bodies; but if with the same body we seek to obtain a great number of images successively its power is seen to grow weak by degrees, and becomes almost nothing after a certain number of impressions, a number which varies with the nature and especially with the texture of the bodies; compact bodies such as the metals rapidly losing this property, porous bodies on the contrary retaining it in a remarkable manner.

"2. When the property of producing images is lost or weakened in a body, it can be instantaneously restored to it by passing the fingers along its surface, or by rubbing this surface with the hairs

* See an account of these discoveries in (vol. iii.) Part XI. of Scientific Memoirs in the three treatises of Moser On the Action of Light on Bodies, On Invisible Light, and On the Power which Light possesses of becoming Latent.

of a living animal, which, as is well known, are always impregnated with organic matters known under the name of grease (*suint*).

"3. When you raise the temperature of the body forming an image, that of the polished surface remaining the same, the image is formed in a very short time.

"4. When a polished surface has received the image of a body, this same surface, placed very near a second polished surface, is capable of forming in its turn an image which may be called *secondary*, and which itself might form tertiary images, if the perfection of the impression did not diminish very quickly by these successive transfers.

"5. When a very thin lamina of mica was interposed between the body forming the image and the polished surface, I have constantly found that the action was null. However, in certain circumstances images are thus obtained which it is of importance not to confound with those that the body itself would have produced; such is the case when the same lamina of mica, serving for two consecutive trials, is placed in the second trial in a position the reverse of that which it had occupied in the first; then the surface of mica which during the first experiment has been in contact with the image-forming body and has thus received an impression, will be in contact with the polished surface during the second, and must thence produce a secondary image. This image may always be distinguished from the direct image, inasmuch as this is evidently a symmetrical representation of the surface of the body, whilst the secondary image being symmetrical, with respect to the preceding one, is found to be an identical representation of the body.

"6. Lastly, the different experiments relative to these images have absolutely the same results, whether we operate under the influence of the light or of total darkness."

Extract from a Letter from M. Knorr, communicated by M. Breguet. *Comptes Rendus*, Feb. 13, 1843, p. 398.

"I have been engaged for these four weeks in following up the discoveries of M. Moser of Kœnigsberg upon invisible light; and have read a short memoir which I had written on the subject at the meeting of our Philosophical Society the 7th (19th) November 1842. I merely related new facts which I had discovered without entering into theoretical speculations, but I believe these facts sufficiently prove that all the actions which Moser attributes to invisible light owe their origin to heat. I have also created an art entirely new, which I have named *thermography*; for I have found that visible images can be obtained without any condensation of vapour on the plates, merely by the action of heat. There are three different methods for this: by the first, images may be obtained in from 8 to 15 seconds, but not with constant success; the second seems applicable only to bodies that are not very good conductors of heat; the third deserves the preference, as ensuring better and almost universal success, but it requires from 8 to 10 minutes to obtain an image. Thus I have received proofs of coins of platinum, gold, silver, engraved plates of copper and brass, engraved stones, steel and glass, also of engravings printed on common paper; the images were formed on plates of silvered copper, pure copper, steel and brass."

ON THE EFFECT OF THE VARIATION OF GRAVITY ON SHIPS' CARGOES IN DIFFERENT LATITUDES. BY A CORRESPONDENT.

Many useful and interesting articles on the mercantile law of shipping not long since appeared in the Law Magazine, No. 32, p. 367; in one of them, the writer, speaking of the master's duties with regard to the ship's cargo, says, "The goods are to be delivered according to the number, *weight*, or measure indicated in the bill of lading. In ascertaining the number no difficulty can exist; in other cases the goods are weighed at the king's beam, or public scale, or are measured by official meters, where such an establishment exists. Not unfrequently serious discrepancies are found to exist between the results so obtained and the quantities specified in the bill of lading; and it is not always easy to determine whether a deficiency is attributable to natural and intrinsic causes, such as evaporation, leakage, shaking, compression, or the like, or to causes for which the owner should be responsible. It is evident however that considerable latitude of allowance should be made, as well on account of variation in the weights and measures of different parts, as of the changes both in bulk and *gravity* to which in the course of a long voyage many commodities are necessarily subject."

The latter part of this quotation suggested the formation of the inclosed tables*. It is well known that all bodies are affected by gravity, and that, in consequence of the earth's being an oblate spheroid, gravity varies in different latitudes: with the view of exhibiting the variation in the weight of a body, in different places, occasioned by the action of gravity, the subsequent table has been formed; it was intended to clear up one of the doubtful points named above. The article has no pretensions to orginality or high scientific merit: how far it may be worth preserving on the score of utility or as a matter of curiosity, the readers of the Philosophical Magazine, should it be inserted, will form their own judgement.

Supposing the earth an oblate spheroid, the axis of which is to its equatorial diameter as 229 : 230, we have the gravitation at the equator to the gravitation at the pole as 230 : 231; and generally the gravitation at the equator is to the gravitation at any place of which the latitude is l, as $230 : 230 + \sin^2 l$. It also follows that the gravitation at a place of which the latitude is l is to the gravitation at any other place the latitude of which is l', as $230 + \sin^2 l : 230 + \sin^2 l'$. Let then W be the weight of a body at the place l and W′ the weight of the same body at the place l',

then $$\frac{230+\sin^2 l'}{230+\sin^2 l} = \frac{W'}{W}, \text{ that is } \frac{230+\sin^2 l'}{230+\sin^2 l} \times W = \text{the}$$

weight of the body at the place of which the latitude is l'.

Let $l = 51° 32'$ the latitude of London, the $\sin^2 l = \cdot 61304$, and the constant denominator of the above fraction is 23061304; the method of obtaining the numerator is obvious, and the decimal multiplier in the table is produced by dividing the one by the other in the usual manner. Hence, if any commodity weigh W at London, its

* We are obliged to omit the tables, from want of room: their construction and use are well explained above.—EDIT.

weight may be found at any other place by multiplying W by the decimal multiplier corresponding to the nearest degree in the table.

Ex. 1.—If a ship's cargo be 1000 tons at London, what will it weigh at Moscow, lat. 55° 45′?

The multiplier corresponding to 56° is 1·000321, which being multiplied by 1000, gives 1000·321 tons, or 1000 tons 6 cwt. 1 qr. 19 lbs., so that the gain or difference in weight is 6 cwt. 1 qr. 19 lbs., which the table indicates.

Ex. 2.—A ship's cargo weighs 500 tons at London, what will be its weight at Madras, lat. 13° 4′?

The decimal multiplier in a line with 13° is ·99756112, which being multiplied by 500, gives 498·78056 tons, so that the cargo is 1 ton 4 cwt. 1 qr. 15$\frac{5}{10}$ lbs. less at Madras than at London.

The table exhibits the gain or loss in weight of all ships from 1000 tons to 100 tons; the gain or loss of almost any other ship may be obtained very easily by the simple rules of arithmetic. Thus the gain of a ship of 1500 tons would be the sum of the gains of the ships marked 1000 tons and 500 tons. The loss of a ship of 50 tons would be half the loss of the ship marked 100 tons, &c.

J. J.

METEOROLOGICAL OBSERVATIONS FOR FEB. 1843.

Chiswick.—Feb. 1. Very fine: cloudy. 2. Heavy rain: overcast. 3. Stormy showers: boisterous. 4. Stormy: very boisterous. 5. Clear and frosty. 6. Cloudy. 7. Hazy: sleet. 8. Dense fog: hazy and cold. 9. Cold easterly haze. 10. Densely clouded. 11. Uniformly overcast. 12. Slight drizzle. 13. Frosty: hazy: sharp frost at night. 14. Frosty: cloudy: severe frost. 15. Sharp frost: snow flakes: frosty. 16. Dry air and frosty: overcast. 17. Clear and frosty: very fine: stormy at night. 18. Stormy, with drifting snow. 19. Overcast: heavy rain. 20. Rain: foggy. 21. Foggy: fine: foggy. 22. Slight rain: cloudy. 23. Very fine. 24. Foggy: cold easterly haze. 25. Slight drizzle: stormy. 26. Sleet: drizzly. 27. Stormy and wet: barometer very low. 28. Cloudy.—Mean temperature of the month 3°·8 below the average.

Boston.—Feb. 1. Fine. 2. Rain: stormy, with rain P.M. 3. Fine: stormy, with snow P.M.: stormy night. 4. Stormy: hail and snow P.M. 5. Fine. 6. Fine: rain and snow P.M. 7. Cloudy: rain P.M. 8. Cloudy. 9. Fine. 10. Fine: rain P.M. 11. Rain: rain early A.M.: rain P.M. 12. Cloudy. 13. Fine. 14—16. Cloudy. 17. Fine. 18. Cloudy: snow A.M. 19. Cloudy: rain P.M. 20. Cloudy: rain early A.M.: rain P.M. 21. Cloudy. 22. Rain: rain early A.M.: rain A.M. 23. Fine. 24. Cloudy. 25. Cloudy: rain and snow P.M. 26—28. Cloudy.

Sandwick Manse, Orkney.—Feb. 1. Showers. 2. At 1½ P.M. wind N.W.: stormy, with drift. 3. Snow-drift: at 12 at night a storm began. 4. Cloudy: thaw: frost. 5. Cloudy: frost. 6. Bright: frost: thaw: aurora. 7. Clear: frost: thaw. 8. Bright: thaw: damp. 9, 10. Cloudy: frost. 11. Drizzly showers: clear hoar-frost. 12. Clear frost: clear hoar-frost. 13. Showers: snow-showers. 14. Snow-drift. 15. Clear: snowing. 16, 17. Snow-showers: clear. 18. Clear: cloudy. 19. Bright: cloudy. 20. Bright: thaw. 21. Cloudy: thaw. 22. Snow-showers: cloudy: frost. 23. Clear and frosty. 24. Bright: cloudy: thaw. 25. Cloudy: frost. 26. Cloudy: snow-showers. 27. Snow-showers: clear and frosty. 28. Clear and frosty: snow-showers.

Applegarth Manse, Dumfries-shire. Feb. 1. Heavy showers P.M. 2. Snow-showers. 3. Snow: frost P.M. 4. Frost and snow. 5. Fine, but frosty. 6. Frost A.M.: rain P.M. 7. Thaw: high wind P.M. 8. Mild and fair. 9. Fair, but chilly. 10. Sprinkling of snow: frost. 11, 12. Fair: no frost. 13. Frost: fine. 14. Frost. 15. Frost: shower of snow. 16—18. Frost. 19. Slight frost. 20. Snow and sleet. 21. Slight rain. 22. Fair. 23, 24. Showery. 25. Fine and fair. 26. Fair, but cloudy. 27. Fair. 28. Frost A.M.: slight snow.

Meteorological Observations made at the Apartments of the Royal Society, London, *by the Assistant Secretary*, Mr. Roberton; *by* Mr. Thompson, *at the Garden of the Horticultural Society at* Chiswick, *near London*; *by* Mr. Veall, *at* Boston; *by the* Rev. W. Dunbar, *at Applegarth Manse*, Dumfries-shire; *and by the* Rev. C. Clouston, *at Sandwick Manse*, Orkney.

Days of Month.	Barometer.								Thermometer.										Wind.					Rain.					Dew-point.
1843. Feb.	London: Roy. Soc. 9 a.m.	Chiswick. Max.	Chiswick. Min.	Boston. 8½ a.m.	Dumfries-shire. 9 a.m.	Dumfries-shire. 9 p.m.	Orkney, Sandwick. 9¼ a.m.	Orkney, Sandwick. 8½ p.m.	London: R.S. Fahr. 9 a.m.	London: R.S. Self-reg. Mx.	London: R.S. Self-reg. Mm.	Chiswick. Max.	Chiswick. Min.	Boston. 8½ a.m.	Dumf.-shire. Max.	Dumf.-shire. Min.	Orkney, Sandwick. 9¼ a.m.	Orkney, Sandwick. 8½ p.m.	London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond.: R.S. 9 a.m.
1.	29·954	29·902	29·833	29·43	29·50	29·38	29·14	28·93	47·8	51·6	45·8	53	45	43	46	47½	40½	38½	s.	w.	calm	nw.	sw.	[illegible]	·16	......	......	[illegible]	6
2.	29·612	29·742	29·612	29·13	29·28	29·21	28·82	28·99	50·5	53·0	48·0	45	37	45	44	44	34	33½	s.	w.	calm	nw.	wsw.	......	......	·12	......	·16	18
3.	29·502	29·517	29·130	29·00	28·85	29·20	28·80	29·27	40·8	51·4	39·2	45	24	37	37	32	31	27½	s.	sw.	w.	nw.	nnw.	[illegible]	·20	......	......	......	41
4.	29·342	29·757	29·348	28·83	29·50	29·80	29·80	29·94	33·6	46·6	35·0	39	30	34	37	20½	35	34	w.	n.	w.	nnw.	n.	[illegible]	[illegible]	......	......	·26	29
5.	29·838	29·854	29·798	29·55	29·80	29·85	29·90	30·04	32·7	39·0	32·6	39	29	30·5	35	28½	31	31	nw.	n.	calm	nnw.	n.	[illegible]	[illegible]	......	......	......	29
6.	29·882	29·868	29·736	29·60	29·94	29·96	30·10	30·20	35·5	37·6	32·0	38	31	32	37	25	36½	36	nw.	n.	calm	nnw.		......	[illegible]	......	[illegible]	......	31
7.	29·986	29·993	29·890	29·74	30·05	30·05	30·40	30·35	34·3	38·7	33·6	37	32	34	41½	33½	34	36	n.	ne.	calm	nne.	se.	[illegible]	·26	·36	......	......	31
8.	30·052	30·038	30·024	29·77	30·03	30·03	30·24	30·17	36·2	37·6	34·2	38	35	36·5	43½	38	37	41	e.	ne.	calm	ne.	sw.	·250	·02	·13	......	......	33
9.	29·976	29·966	29·892	29·70	30·00	30·05	30·30	30·36	37·5	39·4	35·8	39	34	38·5	41	37	33½	34	n.	ne.	calm	ne.	n.	[illegible]	·11	......	......	......	34
10.	29·884	29·881	29·831	29·63	30·05	30·02	30·33	30·29	35·3	40·2	35·2	42	31	35	39	31½	35	35½	n.	ne.	calm	ne.	n.	·160	......	......	......	......	43
11.	29·880	29·976	29·859	29·59	30·07	30·08	30·22	30·32	37·3	40·4	35·3	41	36	37·5	40	34	41½	36	n.	ne.	e.	ene.	n.	......	......	·38	......	·04	33
12.	30·060	30·010	30·030	29·74	30·08	30·05	30·33	30·15	37·7	42·4	37·4	41	27	38	41½	34	31½	36	ne.	ne.	calm	ene.	se.	......	·01	·11	......	......	34
13.	30·014	30·010	29·873	29·70	29·94	29·75	29·84	29·86	35·5	42·2	34·0	42	18	32	37	25	41½	26	e.	e.	calm	e.	w.	......	......	......	0·00	......	33
14.	29·718	29·721	29·613	29·35	29·66	29·58	29·73	29·62	29·8	39·6	28·2	39	16	33·5	34½	26½	24½	24	e.	w.	calm	n.	nw.	......	......	......	......	......	30
15.	29·444	29·470	29·294	29·21	29·40	29·19	29·21	29·28	26·4	39·0	23·8	29	22	25	33	17½	30	27½	n.	n.	calm	n.		......	......	......	......	......	24
16.	29·172	29·234	29·193	29·00	29·15	29·28	29·19	29·45	26·2	29·6	25·8	32	20	24	34	17	32	22	n.	ne.	calm	n.	ese.	......	......	......	......	......	23
17.	29·476	29·502	29·120	29·21	29·45	29·57	29·66	29·79	28·4	32·8	26·4	42	30	24	36	19	32	29	nnw.	ne.	calm	n.	nne.	......	·16	......	......	......	21
18.	29·410	29·425	29·400	29·25	29·60	29·60	29·86	29·84	30·7	36·5	28·6	34	31	35	35½	22	32½	29	ne.	e.	e.	ene.		......	·04	......	......	......	28
19.	29·414	29·355	29·311	29·31	29·51	29·40	29·81	29·79	34·7	35·3	31·5	37	34	36	39	27½	37½	27½	ene.	e.	e.	ene.	ssw.	·033	·28	......	......	......	31
20.	29·272	29·324	29·261	29·04	29·30	29·28	29·76	29·79	37·4	38·3	34·7	39	35	36·5	37	33	35	36½	ene.	e.	e.	e.	e.	·250	·16	·30	......	......	34
21.	29·396	29·371	29·326	29·10	29·27	29·36	29·79	29·79	41·7	42·6	37·3	54	37	38	39	33½	37	36	e.	se.	e.	e.	e.	·175	·12	·15	0·31	·35	36
22.	29·280	29·351	29·246	29·00	29·33	29·30	29·77	29·80	43·3	49·3	41·0	52	41	33	41	35	35½	33	sse.	s.	e.	e.	e.	·077	·02	·10	......	·07	39
23.	29·178	29·519	29·449	29·13	29·35	29·46	29·84	29·87	44·8	50·0	43·5	53	37	40·5	41	34½	34	29	se.	e.	w.	ene.	e.	......	......	·15	......	......	40
24.	29·638	29·640	29·612	29·33	29·55	29·61	29·91	29·90	38·7	49·0	38·0	43	34	38	40½	35	36½	35½	ne.	e.	calm	ene.	e.	......	......	......	......	·05	37
25.	29·618	29·619	29·596	29·35	29·63	29·60	29·83	29·80	37·0	43·6	35·7	38	32	36	40	34½	36	34½	n.	ne.	calm	ene.	se.	......	·26	......	......	......	35
26.	29·606	29·691	29·326	29·37	29·53	29·31	29·75	29·66	34·1	39·0	33·8	40	34	36	37	33	36	34	n.	e.	calm	e.	e.	·350	·03	·06	......	......	35
27.	28·930	28·933	28·848	28·88	29·10	29·05	29·54	29·47	36·8	38·6	34·8	40	37	36	40	33	35	31½	ne.	ne.	calm	ne.	ne.	[illegible]	·19	......	0·35	·08	35
28.	29·022	29·434	29·004	28·83	29·19	29·48	29·56	29·70	38·8	40·4	37·2	41	31	39	41½	32	34	30	nnw.	ne.	calm	e.	ne.	·133	......	......	......	......	35
Mean.	29·602	29·641	29·527	29·31	29·586	29·589	29·765	29·794	36·6	41·6	34·9	41·⅛	31·4	35·3	38·8	30·8	31·73	32·39						Sum. 2·131	2·35	2·16	0·95	1·32	Mean. 33

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LVII. *Experiments on the Heat disengaged in Combinations.* By THOMAS GRAHAM, *Esq., F.R.S., &c.**

THE observations, of which an account shall be given in the present paper, are exclusively confined to the heat disengaged in combinations formed in the humid way. The heat disengaged in such combinations is in general easily collected and measured, as it is immediately communicated to a mass of fluid, of which the temperature may be observed with accuracy. The elevation of temperature in an experiment may often, however, be greatly affected by incidental circumstances; such as the liquefaction of the product of the combination, arising from its solution in the water, or other menstruum employed; or the hydration of the compound formed, which so generally occurs with a salt formed by uniting an acid and base; and can rarely, therefore, be taken as the expression of the heat disengaged from the combination without considerable correction.

Thus in a few preliminary experiments to ascertain whether, as has been anticipated, different bases of the same class evolve equal quantities of heat on combining with the same acid, it was found that equivalents of oxides of copper and zinc, and the equivalent of magnesia on dissolving in highly diluted sulphuric acid, evolved respectively 4°·20, 5°·18, and 11°·70. But the sulphates formed are all hydrated salts, and a large portion of the heat was found to be due to the combination of this water, namely 3°·49 in the sulphate of copper, 3°·90 in the sulphate of zinc, and 4°·12 in the sulphate of magnesia. Again, the salts are obtained in solution; now the liquefaction or solution of salts is attended with the ab-

* Communicated by the Chemical Society; having been read November 1, 1842.

sorption of a certain quantity of heat or fall of temperature, namely by a fall of 0°·66 in the hydrated sulphate of copper, 0°·93 in the hydrated sulphate of zinc, and 0°·83 in the hydrated sulphate of magnesia. The last quantities being added to the heat first observed in the solution of the oxides, and the preceding quantities being subtracted from the same heat first observed, we obtain as the corrected determinations of the heat evolved from the combination with sulphuric acid of the oxides enumerated (or rather from the substitution of these metallic oxides for the basic water of the sulphate of water), by oxide of copper 1°·37, by oxide of zinc 2°·21, by magnesia 8°·41; quantities which, so far from being equal, are nearly in the ratio of the numbers 2, 3, and 12. It is obvious, therefore, that experiments to determine both the heat absorbed in the solution of salts, and that evolved in their hydration, must precede inquiries respecting the heat disengaged in the formation of the salts themselves by the combination of their essential constituents, when the salts are formed in the humid way.

The apparatus employed consisted of a delicate thermometer of small bulb, namely, that used in the wet-bulb hygrometer, as prepared by Greiner of Berlin. Every degree was divided into five parts, each part again was divisible by the eye into five parts, so that observations were made to $\frac{1}{25}$th of a degree. The degree is that of Reaumur's scale. After trying glass jars and various other vessels, it was found that nothing answered better than a large platinum crucible, which weighed 1201·9 grains, and was capable of containing 5 ounces of water. The thermometer and crucible, with a hollow cylinder of palladium, weighing 207·6 grains, employed as a stirrer, were all the apparatus necessary. Of the salt or other substance experimented upon, a quantity corresponding with its atomic weight, and representing a single equivalent, was always used; and the quantity of water was constant, namely 1000 grains, and relatively large, so as to render the change of the specific heat of the fluid insensible.

The water, crucible, stirrer and thermometer, being the same in all the experiments, the results are strictly comparable. The numbers express the relative quantities of heat disengaged from atomic equivalents of the bodies.

I. *Hydration of Oil of Vitriol.*

1. H O, S O. The protohydrate of sulphuric acid employed was pure, and of density 1·848. The quantity used of this and other substances was always one-twentieth of the number expressing the equivalent taken in grains; that is 30·68

grains of oil of vitriol, the equivalent of the protohydrate being 613·5. It was weighed in an exceedingly thin and light glass spherule, which was afterwards broken in the water, and the acid diffused through the latter. The greater portion of the heat is disengaged in the first two or three seconds after mixture, or its evolution is almost instantaneous. To avoid the loss of heat by communication to the air, during the short time that must elapse before the thermometer in the liquid becomes stationary, the crucible, water and stirrer were previously cooled down so far below the temperature of the air, as the liquid was expected, from a preliminary experiment, to rise on the addition of the acid. The crucible was also placed within a glass jar containing tow, to impede the passage of heat by conduction. I am indebted for several valuable hints on the mode of conducting such experiments, to the papers of Dr. Andrews* and Professor Hess†, who have preceded me in similar investigations.

The rise of temperature in a preliminary experiment, in which the water and crucible were not previously cooled, was $3^{\circ}\cdot78$ R. In two other experiments in which the crucible and water were previously cooled before the addition of the acid, the rise was $3^{\circ}\cdot88$ and $3^{\circ}\cdot85$. The mean of the last results, or $3^{\circ}\cdot86$, may therefore be taken as the heat disengaged in the hydration of an equivalent of the protohydrate of sulphuric acid. No perceptible change of temperature occurred on diluting further with water the products of these experiments.

2. $HO, SO_3 + HO$. This is the crystallizable hydrate of sulphuric acid, of density 1·78. 36·3 grains, the equivalent quantity, were mixed with 1000 grains of water, as in the preceding case. The rise of temperature in three experiments was $2^{\circ}\cdot40$, $2^{\circ}\cdot36$ and $2^{\circ}\cdot40$; of which the mean is $2^{\circ}\cdot39$.

The dilution of this hydrate gives occasion to the disengagement of $1^{\circ}\cdot47$ less heat than the preceding hydrate. It appears, therefore, that in the dilution of the first hydrate or of sulphate of water, $1^{\circ}\cdot47$ is due to the combination of the first atom of water, with which it forms the crystallizable hydrate, and $2^{\circ}\cdot39$ to combination with all the rest, making together $3^{\circ}\cdot86$.

3. $HO, SO_3 + 2HO$. This is the hydrate of sulphuric acid in the formation of which the greatest contraction is observed to occur. With 41·93 grains, or one equivalent, the rise of temperature on dilution was in three experiments conducted as before, $1^{\circ}\cdot88$, $1^{\circ}\cdot86$ and $1^{\circ}\cdot85$, of which the mean is $1^{\circ}\cdot86$. The difference between the heat evolved by the present and the immediately preceding hydrate is $0^{\circ}\cdot53$, which

[* See Phil. Mag. S. 3. vol. xix. p. 183.] [† *Ib.* p. 19.]

is therefore the heat evolved by the addition of the second atom of water to the sulphate of water. It is scarcely one-third of 1°·47, the quantity evolved by the first atom.

4. $HO, SO_3 + 3 HO$. With 47·55 grains, or one equivalent of this hydrate, the rise by dilution was in three experiments 1°·31, 1°·31 and 1°·27, of which the mean is 1°·30. The difference between the heat evolved by this and the preceding hydrate is 0°·56, which is therefore the heat evolved by the addition of the third atom of water to the sulphate of water. Now the second atom evolved 0°·53, so that the second and the third atoms of water appear to evolve sensibly the same quantity of heat. This curious result favours the conclusion, that the second and third atoms of water go together; or that the hydration of the sulphate of water here advances by two atoms at a time, and that no intermediate hydrate exists, in a state of solution at least, between $HO, SO_3 + HO$ and $HO, SO_3 + 3 HO$. The last may be represented as $HO, SO_3, HO + 2 HO$.

5. $HO, SO_3 + 4 HO$. With 53·18 grains, or one equivalent of this hydrate, the rise of temperature on dilution was 1°·05, 1°·07 and 1°·05; mean 1°·06. The difference between this and the preceding hydrate, namely 0°·24, is therefore the heat evolved by the combination of the fourth atom of water with sulphate of water.

6. $HO, SO_3 + 5 HO$. With 58·8 grains, or one equivalent of this hydrate, the rise of temperature on dilution was 0°·88, 0°·88 and 0°·85; mean 0°·87. The heat from the combination of the fifth atom of water is 0·19. It is not impossible that the heat evolved from the fifth is the same in quantity as that evolved from the fourth atom of water, and that these two atoms go together like the second and third. The present hydrate of sulphate of water corresponds with crystallized sulphate of copper.

7. $HO, SO_3 + 7 HO$. With 70·05 grains, or one equivalent, the rise was 0°·68, 0°·71 and 0°·65; mean 0°·68. The difference between the effect of this and the preceding hydrate is 0°·19, which is therefore the heat evolved by the combination of the last two atoms of water, namely the sixth and seventh atoms. This hydrate of sulphate of water corresponds with crystallized sulphate of magnesia.

By the continued hydration of the sulphate of water, the quantities of heat evolved are therefore as follows:—

	Heat evolved.		Hydrate formed.
By first atom of water	1°·47	...	$HO, SO_3 + HO$.
By second and third atoms together .	1°·09	...	$HO, SO_3 + 3 HO$.
By fourth and fifth atoms together . .	0°·43	...	$HO, SO_3 + 5 HO$.
By sixth and seventh atoms together .	0°·19	...	$HO, SO_3 + 7 HO$.
By an additional excess of water . . .	0°·68	...	$HO, SO_3 + 7 HO + x HO$.

It will be observed that the heat evolved by the first atom is sensibly the same as that evolved by the four following atoms, the quantities being 1°·47 and 1°·52; the difference between these numbers being within the limits of errors of observation. The same conclusion is drawn from his experiments on the hydration of oil of vitriol by Professor Hess. Supposing the whole heat disengaged in the hydration of sulphate of water to be divided into 23 parts, 9 are evolved by the first atom of water, 9 by the next four atoms, 1 by the following two atoms, and 4 by the remaining excess.

Although the experiments detailed above agree with those of M. Hess in bringing out one curious result, they yet differ from them to an extent which it is difficult to account for in other respects. Thus reducing my results to the same scale as those of M. Hess, the comparison is as follows. In the hydration of the sulphate of water,

	Hess.	Graham.
Heat from the first atom of water . . .	2	2·
... second atom of water . .	1	0·72
... next three atoms of water	1	1·35
... additional excess of water	1	1·18
	5	5·25

8. $HO, SO_3, HO + 10 HO$. An equivalent quantity of this hydrate, or 92·55 grains, was mixed with 969·3 grains of water, the quantity of the latter being diminished so as to make up 1000 grains with the water already in the acid hydrate. The rise of temperature in two experiments was 0°·37 and 0°·41, of which the mean is 0°·39. This hydrate contains four atoms more of water than the last operated upon, and disengages 0°·29 less heat. The heat, therefore, due to the combination of the additional four atoms of water is 0°·29.

9. $HO, SO_3, HO + 14 HO$. Of this hydrate the equivalent, or 115·05 grains, was mixed with 915·6 grains of water, and occasioned a rise of temperature in two experiments of 0°·23 and 0°·20, of which the last was believed to be the most trustworthy result. Hence the four atoms of water last added evolve 0°·09, or about one-third of the quantity evolved by the preceding four atoms of water.

10. $HO, SO_3, HO + 24 HO$. The equivalent of this hydrate, or 171·3 grains, was mixed with 859·4 grains of water, and produced in one experiment a rise of 0°·15. The hydrate was kept for three days before it was diluted in the experiment; for immediately after its preparation the heat which this hydrate yielded on dilution was considerably less than the quantity assigned above to it; indeed not more than 0°·06 in one experiment.

11. $HO, SO_3, HO + 36\ HO$. The equivalent quantity of this diluted acid, or 238·8 grains, was mixed within an hour of its preparation with 792 grains of water; the rise of temperature was 0°·11.

12. $HO, SO_3, HO + 48\ HO$. The equivalent quantity, or 306·3 grains, was mixed about three hours after its preparation with 724·4 grains of water; a rise occurred of 0°·08. The dilution of the same hydrate twenty-four hours after its preparation was attended with a rise of 0°·13.

The last hydrate is oil of vitriol diluted with nine times its weight of water, yet it was still capable of evolving a sensible quantity of heat by further dilution. The term at which the mixture of acid and water ceases to disengage heat on a further addition of water, was not observed, but the effect was insensible in a mixture formed of one part of the concentrated acid and thirty parts of water.

II. *Hydration of other Magnesian Sulphates.*

The heat produced in the hydration of different anhydrous sulphates, compared with oil of vitriol, appears in the following results; equivalent quantities of the anhydrous salts in the solid state being thrown into the same quantity of water, and the rise of temperature observed after the hydration and complete solution of the salts.

Protosulphate of manganese	3°·22
Sulphate of copper . . .	3°·73
Sulphate of water . . .	3°·86
Sulphate of zinc	4°·17
Sulphate of magnesia . .	4°·33

The most material difference in the circumstances of the experiments is, that while the oil of vitriol was liquid, the salts with which it is compared were necessarily applied in the solid form. The liquefaction of the latter during the experiment, would therefore occasion an absorption of heat of unknown amount, which does not occur in the latter.

1. *Sulphate of Magnesia.*—The same mode of experimenting was followed and apparatus used as in the preceding experiments with oil of vitriol. On dissolving the equivalent quantity, 77·35 grains (one-twentieth of 1547·02), of the crystallized salts in 960·6 grains of water, a fall occurred in three experiments of 0°·96, 0°·90 and 0°89, of which the mean is 0°·92. In these experiments, the water contained in the crystals, which amounts to 39·4 grains, was deducted from the 1000 grains of water usually employed to dissolve the salt; but if this quantity of water is supposed to be added, the mean result would become 0°·88.

The salt was made certainly anhydrous by exposure to an incipient red heat for a considerable time, and the equivalent quantity, 37·98 grains, in the state of a fine powder, was thrown into 1000 grains of water. It did not cake, and was dissolved completely by stirring in about one minute and a half. The rise of temperature in two experiments was 4°·30 and 4°·36, of which the mean is 4°·33. To this must be added the heat lost by the liquefaction and solution of the hydrate formed.

Rise on solution of $Mg\,O,\ S\,O_3$	4°·33
Fall from solution of $Mg\,O,\ S\,O_3 + 7\,H\,O$	0°·92
Whole heat disengaged by $Mg\,O,\ S\,O_3$.	5°·25

$Mg\,O,\ S\,O_3,\ H\,O$. It is not easy to obtain the sulphate of magnesia with exactly one atom of water. The salt first operated upon retained, after being dried by an oil-bath, at 400° to 100 sulphate of magnesia only 14·14 water, instead of 14·81, the single equivalent. The hydrate was therefore $\frac{21}{22}$ HO. The heat evolved by the solution of 43·35 grains, an equivalent quantity of this hydrate in two experiments, was 3°·06 and 3°·9, of which 3°·08 may be taken as the mean.

Another portion of the same sulphate less strongly dried, retained to 100 sulphate of magnesia 15·75 water, which is $1\frac{1}{17}$ H O. The results from the solution of 43·93 grains, the equivalent of this hydrate, were 3°·03, 2°·98 and 2°·93, of which the mean is 2°·98. The mean of the two sets of experiments, or 3°·03, probably does not differ far from the truth.

Rise on solution of $Mg\,O,\ S\,O_3,\ H\,O$ . .	3°·03
Fall on solution of $Mg\,O,\ S\,O_3, + 7\,H\,O$.	0°·92
Whole heat disengaged by $Mg\,O,\ S\,O_3,\ H\,O$	3°·95

The anhydrous salt disengaged 5°·25, while the protohydrate disengages 3°·95; the difference, or 1°·30, is therefore the heat disengaged by the combination of the first atom of water with sulphate of magnesia. It thus appears that of the whole heat evolved in the complete hydration of sulphate of magnesia, as nearly as possible one-fourth is due to the combination of the first atom of water, one-fourth of 5°·25 being 1°·31.

2. *Sulphate of Zinc.*—The equivalent quantity of the crystallized salt, 89·59 grains, contains 39·38 water, and was therefore dissolved in 960·6 water. The fall of temperature in two experiments was 1°·01 and 0°·98, of which the mean is 1°·00. This sensibly exceeds the cold produced by the solution of crystallized sulphate of magnesia, which is 0°·92. The difference has a real foundation, and is not the consequence of errors in experiment; for in two other sets of observations on the same salts made in glass, and which may be compared with each other, although not with the preceding experiments,

the results were for sulphate of magnesia 0°·85, 0°·80 and 0°·83, of which the mean is 0°·83; for sulphate of zinc 0°·97, 0°·91, 0°·92, of which the mean is 0°93: greater cold occasioned by the solution of sulphate of zinc than of sulphate of magnesia, by the first experiments 0°·08, by the last experiments 0°·10.

Of sulphate of zinc, carefully dried and made perfectly anhydrous, the equivalent quantity, 50·22 grains, was dissolved in 1000 grains of water, with the exception of a mere trace of flaky matter. The rise in one experiment was 4°·20; in another 4°·15; mean 4°·17. The results then are,—

Rise on solution of ZnO, SO_3	4°·17
Fall on solution of $ZnO, SO_3 + 7HO$.	1°·00
Whole heat disengaged by ZnO, SO_3 . .	5°·17

There is the same difficulty in obtaining the protohydrate of sulphate of zinc exactly definite, as the corresponding hydrate of sulphate of magnesia. The hydrate operated upon contained to 100 sulphate of zinc 11·99 water, instead of 11·207, which is a single equivalent. The equivalent quantity, 56·21 grains, was dissolved in 1000 grains of water, and occasioned a rise of temperature in two experiments of 2°·34 and 2°·33. As the rise for the anhydrous salt was 4°·17, the deficiency from the hydrate, $4·17 - 2·34 = 1°·83$, is due to the quantity of water already combined in the salt of the experiment. But this deficiency cannot be entirely ascribed to a single atom of water, as the combined water exceeded that proportion as 11·99 to 11·21. It is difficult to find proper elements for the necessary correction, but we may probably reduce the amount of deficient heat to 1°·71, that is, as 11·99 to 11·21, without any considerable error. Hence

Rise on solution of ZnO, SO_3, HO . .	2°·45
Fall on solution of $ZnO, SO_3 + 7HO$.	1°·00
Whole heat disengaged by ZnO, SO_3, HO	3°·45

The difference between the heat disengaged by the protohydrate and the anhydrous salt, or the heat due to the combination of the first atom of water, namely 1°·71, is almost exactly one-third of the whole heat disengaged in the hydration of sulphate of zinc; one-third of 5°·17 being 1°·72. The quantities of heat disengaged by sulphate of zinc in the two conditions specified, are therefore as 4 to 6.

3. *Sulphate of Copper.*—The equivalent quantity of the ordinary crystallized salt, containing 5 HO, namely 77·97 grains, was dissolved in 1000 grains of water, with a fall of temperature in three experiments of 0°·67, 0°·65 and 0°·68, of which the mean is 0°·67. This and other more sparingly

soluble salts were pounded fine and sifted; the solution took place with stirring within one minute.

Of the anhydrous salt, 49·84 grains, the equivalent quantity, were dissolved in 1000 grains of water, with a rise in two experiments of 3°·72 and 3°·74. Hence the results for the anhydrous salt are,—

Rise on solution of $Cu\,O,\ S\,O_3$	3°·73
Fall on solution of $Cu\,O,\ S\,O_3 + 5\,H\,O$.	0°·67
Whole heat disengaged by $Cu\,O,\ S\,O_3$ . .	4°·40

The protohydrate was prepared by drying the crystallized salt by a nitre-bath; it retained to 100 sulphate of copper 11·83 water, instead of 11·29 water, the single equivalent. The equivalent quantity, 55·72 grains, was dissolved in 1000 grains of water, with a rise in two experiments of 2°·15 and 2°·13. The result is 3·73 − 2·14 = 1°·59 for the combined water. This hydrate contained $1\frac{1}{22}$ H O.

After being dried still further on an oil-bath at 370°, it consisted of 100 sulphate of copper and 11·44 water, or was $1\frac{1}{75}$ H O; the salt was now almost white, the green tint being barely perceptible. The equivalent quantity of the last salt, 55·54 grains, was dissolved in 1000 grains of water, but somewhat more slowly and with greater difficulty than the preceding salt. The heat evolved in two experiments was 2°·09 and 2°·07, which instead of exceeding falls short of the preceding results. The deficiency of heat in the last experiments is remarkable, and is in some measure, but I believe not fully, accounted for by the slowness of the solution. Giving a preference to the first results, and deducting $\frac{1}{22}$nd part for the excess of water above one atom already combined with the salt, there remains 1°·47 for the heat due to the combination of the first atom of water. The result for the protohydrate is,—

Rise on solution of $Cu\,O,\ S\,O_3,\ H\,O$ . .	2°·26
Fall on solution of $Cu\,O,\ S\,O_3 + 5\,H\,O$.	0°·67
Whole heat disengaged by $Cu\,O,\ S\,O_3,\ H\,O$	2°·93

One-third of 4°·40, the whole heat evolved in the hydration of sulphate of copper, is 1°·466, which is as nearly as possible the result obtained above for the first atom of water. The ratio is the same as in the sulphate of zinc, while in the hydration of the sulphate of magnesia the heat evolved by the first atom of water was one-fourth of that evolved by the whole. It may be inferred from the experiments on oil of vitriol, that it approaches more closely to the former salts than to sulphate of magnesia in this character, although a rigid comparison cannot be made, as we are unacquainted

with the fully hydrated sulphate of water in a crystalline form, and cannot therefore estimate its heat of liquefaction.

4. *Protosulphate of Iron.*—Of the crystallized salt containing seven atoms of water, the equivalent quantity, 86·39 grains, dissolved in 1000 grains of water in two experiments with a fall of 1° and 1°·04. Allowing for the 39·38 grains of water introduced by the salt in addition to the thousand grains employed, these results become 1°·04 and 1°·08, of which the mean is 1°·06.

Fall on the solution of $Fe\,O, S\,O_3 + 7\,H\,O$... 1°·06.

The protohydrate of sulphate of iron, formed by drying the crystallized salt in air at a temperature approaching 400°, was found to be nearly insoluble in cold water. The anhydrous sulphate was more soluble, but not sufficiently so for the determination of its thermal relations.

5. *Protosulphate of Manganese.*—The crystallized salt employed contained five atoms of water. The equivalent quantity, 75·47 grains, of the crystallized salt was dissolved in 972 grains of water at 59° Fahr., with a fall of temperature in two experiments of 0°·11 and 0°·13 R., of which the mean is 0°·12.

Of the same salt made anhydrous by heat, the equivalent quantity, 47·35 grains, was dissolved in 1000 grains of water at 60° Fahr., with a rise in two experiments of 3°·20 and 3°·24 R.; mean rise 3°·22.

Rise on solution of $Mn\,O, S\,O_3$	3°·22
Fall on solution of $Mn\,O, S\,O_3 + 5\,H\,O$.	0°·12
Whole heat disengaged by $Mn\,O, S\,O_3$.	3°.34

The crystallized salt being well dried at a temperature not exceeding 400° Fahr., was found to retain a quantity of water in combination, which slightly exceeded a single equivalent, namely in the proportion of 5·82 grains to 5·62 grains, in 52·97 grains of the hydrated salt. The heat evolved in the solution of the equivalent quantity, 52·97 grains, of this protohydrate by 1000 grains of water was in two experiments 1°·80 and 1°·78, of which the mean is 1°·79.

Rise on solution of $Mn\,O, S\,O_3, H\,O$ . .	1°·79
Fall on solution of $Mn\,O, S\,O_3 + 5\,H\,O$.	0°·12
	1°·91

It follows that the heat evolved by the combination of the first atom of water with sulphate of manganese is 3°·34 − 1°·91 = 1°·43. This result approaches to 1°·47, the heat evolved by the combination of the first atom of water with sulphate of copper. The small depression of temperature produced by the solution of crystallized protosulphate of manganese is re-

markable, and distinguishes this salt from the other magnesian sulphates. This salt alone of the class forms a thick solution, when highly concentrated, and crystallizes with difficulty. It was also observed that the protohydrate of sulphate of manganese does not dissolve easily in cold water; the quantity of the protohydrate employed in the experiments narrated above requiring to be agitated with the water for two and a half minutes, before the liquid ceased to be turbid and the salt was entirely dissolved. The anhydrous sulphate of manganese was dissolved quickly and with ease.

III. *Sulphates and Chromates of the Potash family.*

1. *Sulphate of Potash.*—Good crystals of this salt were reduced to powder and sifted. The solution of the equivalent quantity, 54·55 grains, in 1000 grains of water, which took place in thirty seconds, was attended by a fall of temperature in two experiments of 1°·50 and 1°·52, of which the mean is 1°·51.

Fall on solution of KO, SO_3 . . . 1°·51.

The same quantity of sulphate of potash was dissolved in a mixture of 300 water-grain measures of dilute sulphuric acid of density 1·1 mixed with 700 grains of water. The dry acid in the mixture amounted to 36 grains; a single equivalent is represented by 25 grains. The solution was quite as rapid, or more so, than in pure water; the fall of temperature 2°·04; the difference of 0°·53 is probably connected with the formation of bisulphate of potash.

2. *Chromate of Potash.*—The solution of the equivalent quantity, 62·09 grains, of this salt in 1000 grains of water, was attended with a fall of 1°·18 in water.

Fall on solution of KO, CrO_3 . . . 1°·18

When dissolved in an equal quantity of the same dilute sulphuric acid as was used with sulphate of potash, the solution became red from the formation of bichromate, and only a very slight change of temperature occurred, namely a fall of 0°·08.

3. *Bichromate of Potash.*—The fused salt was used, as it is easily reduced to a fine powder, and half the equivalent quantity used, as the whole equivalent is not dissolved by 1000 grains of water at 57° Fahr., the temperature of the experiments. The solution of 47·34 grains, half the equivalent quantity, was attended with the same fall of 1°·98 in two experiments. No sensible change of temperature occurred on diluting this solution. In the dilute sulphuric acid used with the two preceding salts, the fall on the solution of half an equivalent of bichromate of potash was 2°·00, or sensibly the same as in

pure water. The fall of temperature for a whole equivalent of bichromate of potash will therefore be 3°·96.

Fall on solution of $K O, 2 Cr O_3$ . . 3°·96

The heat of liquefaction of bichromate of potash is therefore very considerable. It appears to be the same in quantity as that of *nitrate of potash.* The equivalent quantity of the latter salt, 63·25 grains, was dissolved in 1000 grains of water, with a fall of 3°·86. The temperature of this solution was further reduced 0°·10, by dilution with another 1000 grains of water; so that by the solution of an equivalent quantity of this salt in the same proportion of water as was employed for the solution of an equivalent of bichromate of potash, a fall of temperature of 3°·96 is produced. In a second experiment the whole fall of temperature on the solution of an equivalent of nitrate of potash was 3°·95.

Fall on solution of $K O, N O_5$ . . . . . 3°·96

It is possible that this coincidence is not accidental, but depends on a thermal equivalency of $N O_5$ and $Cr_2 O_6$, the acids united with potash in these two salts. If the single equivalent of nitrogen in nitric acid be divided by three, or considered three atoms instead of one, as has been inferred on other grounds, then the acid constituents of both salts will contain the same number of atoms, namely eight; and the bichromate of potash, which has hitherto appeared so anomalous among salts, be assimilated to the nitrate of potash.

4. *Terchromate of Potash.*—Of this salt 63·63 grains, or one-half of the equivalent quantity, were dissolved easily and entirely by 1000 grains of water, with a fall of 1°·63. But the terchromate of potash changes colour when thrown into water from decomposition, being resolved in a great measure into bichromate of potash and chromic acid, both of which are soon dissolved, the last more rapidly than the first.

Half an equivalent of this salt was dissolved, with a fall of 1°·28, in 1000 water-grain measures of dilute nitric acid, of specific gravity 1·1453. But in this menstruum also, the terchromate appeared to be decomposed with separation of chromic acid, although to a much less extent than in the preceding experiment. In a liquid, however, already charged with the salt, like the last, an additional quantity may be dissolved without further decomposition. Half an equivalent of the salt was dissolved in that liquid with a fall of 1°·14, which is a fall of 2°·28 for a whole equivalent of the salt. The capacity for heat of the solution in question does not (I believe) differ materially from that of 1000 grains of water.

Fall from solution of $K O, 3 Cr O_3$ . . . 2°·28

Half an equivalent of the crystallized *biphosphate of potash*, or 42·68 grains, was dissolved in 1000 grains of water, with a fall of 1°·12, which gives 2°·24 for the whole equivalent.

Fall from solution of $2\,HO \,.\, KO, PO_5$. 2°·24

A corresponding proportion of the crystallized *binarseniate of potash*, or 56·38 grains, were dissolved by 1000 grains of water, with a fall in one experiment of 1°·13, and in another of 1°·18. In a third experiment the solution of a whole equivalent of this salt, or 112·75 grains, was attended by a fall of temperature of 2°·15. A greater discrepancy is observable in the results obtained from this than from most other salts, which appeared to arise from the full depression of temperature not occurring at the moment of solution, but a small portion of it being produced in a gradual manner for three or four minutes after the solution. The mean of the three observations gives 2°·26 for the equivalent quantity of the salt.

Fall from solution of $2\,HO \,.\, KO, As\,O_5$. 2°·26

The thermal properties of these two salts are interesting in relation to the terchromate of potash. The latter salt contains 14 atoms, which is also the number of atoms in both biphosphate and binarseniate of potash, if the equivalents of phosphorus and arsenic be supposed, like that of nitrogen, to represent three atoms.

Potash being common to the terchromate and biphosphate of potash, there remain, on subtracting that constituent from both salts, three equivalents of chromic acid equivalent in some sense to one equivalent of phosphoric acid together with two equivalents of water. This statement respecting phosphoric acid, recalls the view which has lately been proposed by M. Wurtz of the constitution of the hypophosphites, in which the two atoms of water which they all contain are supposed not to be basic, but to form part of the acid; a neutral hypophosphite being represented by $RO + PO, H_2O_2$, or rather by $RO + PO_3\,H_2$. For we are here representing biphosphate of potash as $KO + PO_7\,H_2$, corresponding with the terchromate of potash $KO + Cr_3\,O_9$, in which P is equivalent to Cr_3, and $O_7 + H_2$ to O_9. The two atoms of water, however, may be replaced by a strong base in a biphosphate, but not in a hypophosphite. The relations of these salts show a progressive and imperceptible passage of the basic elements of a salt into constituents of its acid, and the existence of intermediate conditions of the elements in question, which we may well conceive although our chemical formulæ fail to enable us to denote them; these formulæ being adapted only for the expression of the extreme conditions.

Of anhydrous *chromic acid* an equivalent, 32·59 grains, was dissolved by 1000 grains of water with a rise of 0°·51. A second equivalent, dissolved in the previous solution, produced a rise of only 0°·38. The relations of this acid to water are therefore very different from those of sulphuric acid.

5. *Sulphate of Soda.*—In removing the hygrometric water which the crystals of this salt generally contain in large quantity, by pressure in blotting-paper, the salt is apt to lose a little of its combined water. The crystallized salt contained as determined by analysis, to 100 sulphate of soda, 121·5 water, instead of 126·1 water, which are ten equivalents. The equivalent quantity of the fully hydrated salt is 100·85 grains, but of the salt under examination only 98·79 grains. The last quantity, which contains 54·2 grains of water, was dissolved in 946 grains of water in half a minute, with a fall of 4°·43. The fall is almost entirely due, as will immediately appear, to the liquefaction of the combined water of the salt, of which the quantity liquefied in the experiment was 54·2 grains instead of 56·2, the ten equivalents. The fall of 4°·43 increased in the proportion of 54·2 to 56·2, becomes 4°·59.

Fall on solution of $Na\,O,\,S\,O_3 + 10\,H\,O$ . . 4°·59.

The same quantity of the salt was dissolved in the diluted sulphuric acid of the experiments with the previous salts, with a fall of 5°·00; which, corrected in the same manner as the last result, gives a fall of 5°·19 for the equivalent of the salt. Hence the fall on the solution of the sulphate of soda in dilute sulphuric acid is 0°·60 greater than in pure water; a circumstance connected probably with the formation of bisulphate of soda.

Sulphate of soda was made anhydrous by a strong heat, without being fused. The solution of the anhydrous salt is difficult, owing to the instantaneous formation of a hard coherent mass when the salt is thrown into water, which it requires two or three minutes to break up and dissolve. Very little change of temperature occurs. A rise took place in one experiment of 0°·10. In another experiment, in which the salt was added in a gradual manner with constant stirring, there was less caking, and the solution more rapid, although it still required two minutes. A rise occurred of 0°·18. The last experiment is most to be depended upon. The results for the sulphate of soda will therefore be,—

Rise on solution of $Na\,O,\,S\,O_3$	0°·18
Fall on solution of $Na\,O,\,S\,O_3 + 10\,H\,O$.	4°·59
Whole heat disengaged by $Na\,O,\,S\,O_3$ . .	4°·77

The last number represents the heat evolved in the formation

of a solid hydrate of sulphate of soda containing ten atoms of water; it is remarkable how little it exceeds the heat disengaged in the crystallization of the same salt, or the fall observed on the solution of the crystallized salt. It appears as if water abandoned little more than its heat of fluidity on combining with dry sulphate of soda to form a solid hydrate.

Sulphate of soda, which had been allowed to effloresce in dry air between 50° and 55° Fahr. for a week, consisted of dry salt 100 and water 0·46. The equivalent quantity of this salt, which is so nearly anhydrous, or 44·81 grains, was dissolved in 1000 grains of water with a very slight change of temperature, namely a rise of 0°·05.

6. *Sulphate of Ammonia.*—Of the hydrated salt crystallized by spontaneous evaporation in air, which contains one atom of water of crystallization, the equivalent quantity, 47·03 grains, was dissolved in 1000 grains of water with a fall of temperature in three experiments of 0°·65, 0°·64 and 0°·61, of which the mean is 0°·63.

Fall on solution of $N\,H_4\,O, S\,O_3 + H\,O$. 0°·63.

The salt was obtained anhydrous by drying at 248° Fahr.; it was granular and crystalline, and neutral to test paper. The equivalent quantity, 41·41, produced a fall in three experiments of 0°·51, 0°·53 and 0°·49; of which the mean is 0°·51.

Fall on solution of $N\,H_4\,O, S\,O_3$ . . . 0°·51.

A sensible but very small reduction of temperature, not exceeding 0°·02, occurred on mixing the solution of sulphate of ammonia with an equal bulk of water at the same temperature.

Dissolved in the diluted acid, consisting of a mixture of 300 water-grain measures of sulphuric acid of density 1·1 and 700 grains of water, the equivalent, 41·41 grains, of the anhydrous salt produced a fall of temperature in two experiments of 1°·17 and 1°·14; of which the mean is 1°·16. Hence the fall is greater on the solution of the sulphate of ammonia in dilute sulphuric acid than in water, by 0°·65. The fall of sulphate of soda was also greater by nearly the same amount, 0°·60, and of sulphate of potash by 0°·53, when these salts were dissolved in the same dilute acid instead of water.

IV. *Double Sulphates.*

1. *Bisulphate of Potash.*—Of the usual double sulphate of water and potash crystallized in rhombohedral crystals, an equivalent quantity, 85·23 grains, was dissolved in 1000 grains of water, with a fall in two experiments of 1°·96 and 1°·95. The same salt was fused by heat and pounded; it dissolved

afterwards with a fall of 1°·94 and 1°·90 in two experiments. The cold upon solution of this salt appears to be the same before and after fusion. The result is,—

Fall on solution of $H\,O, S\,O_3 + K\,O, S\,O_3$. 1°·95.

I was anxious to compare with this salt the anhydrous bisulphate of potash of M. Jacquelin, which is described as being capable of dissolving in water without decomposition. One equivalent of sulphate of potash was accordingly dissolved in two equivalents of oil of vitriol, with the aid of heat, and an abundant crop was obtained on cooling of a salt in small silky crystals. As these appeared to be the salt in question, an equivalent quantity, or 79·60 grains, was dissolved, and a fall observed of 1°·90. The result not differing from that of the former salt, the preparation of the anhydrous salt was repeated. The spongy mass of thin prismatic crystals obtained in a second experiment was pressed, dissolved again in water, crystallized and pressed again. The salt was still in minute prisms. The solution of 39·8 grains, half the equivalent quantity, was attended with a fall in two experiments of 0°·91 and 0°·95; or, for a whole equivalent, 1°·86. Of the same salt, before the second solution, half an equivalent produced a fall of temperature of 0°·96; or, for the whole equivalent, 1°·92. These results are identical with those formerly obtained with the hydrated bisulphate, if allowance be made for the smaller quantity of the salt employed, a circumstance which excited a doubt as to the composition of the prismatic salt. The product of the second crystallization was accordingly analysed; 19·30 grains of it gave 32·34 grains of sulphate of barytes, equivalent to 11·12 sulphuric acid, or 57·59 per cent.; 22·53 grains of the crystals lost no weight at 150°, but lost 0·24 water, or 1·02 per cent., by cautious fusion. The proportion of acid in the salt is greatly under that of an anhydrous bisulphate, namely 62·98 per cent., while it approaches sufficiently near that of the hydrated bisulphate, 58·74 per cent. The process of M. Jacquelin has not therefore given an anhydrous bisulphate of potash in my hands, and none of my experiments favours the existence of such a salt; the silky prismatic crystals which I obtained being nothing more than an unusual form of the sulphate of water and potash.

2. *Bisulphate of Soda.*—An equivalent quantity, 75·27 grains, of one and the same specimen of this salt, dissolved in three experiments with a fall of 0°·40, 0°·28 and 0°·17. It did not dissolve so easily as the bisulphate of potash, possibly from partial decomposition and formation of a portion of neutral sulphate of soda. The same supposition will explain the want

of agreement among the results. Taking the mean of the results,—

Fall on solution of $H\,O, S\,O_3 + Na\,O, S\,O_3$. $0^\circ{\cdot}28$.

The fall of temperature observed on dissolving bisulphate of potash in water approaches that observed on dissolving the neutral sulphate of potash in dilute sulphuric acid, the first being $1^\circ{\cdot}95$ and the second $2^\circ{\cdot}04$. But it is doubtful if the fall in the second case can be ascribed simply to the immediate formation and solution of bisulphate of potash, when the sulphate of potash and dilute sulphuric acid are mixed and dissolved together. In the formation of bisulphate of potash we have both the substitution of sulphate of potash for the second atom of water of the sulphate of water, and the throwing off of all the remaining water combined with the sulphate of water in hydrated sulphuric acid, bisulphate of potash containing no water of crystallization. Now as a great deal of heat was disengaged by this additional water on originally combining with the sulphate of water, we should expect heat again to be assumed by that water on becoming free, or cold to be produced.

3. *Sulphate of Magnesia and Potash.*—An equivalent quantity of the crystallized salt, namely 126·28 grains, containing 33·75 grains of water of crystallization, was dissolved in 976·2 grains of water at 52° Fahr., with nearly two minutes' stirring; a fall of temperature was observed of $2^\circ{\cdot}30$ R. The experiment was repeated with the same result. But a slow rise of temperature was afterwards observed to occur in the solution, independent of any external influence, which in the course of four minutes amounted to $0^\circ{\cdot}20$ R. This is not the only salt in which the fall of temperature on solution is immediately followed by a slight but sensible rise.

This salt was made anhydrous by a low red heat to which it was exposed for upwards of two hours, but was not fused. When the salt was thrown into water after this ignition, it gave a liquor which remained white and milky for two or three minutes, but the salt finally dissolved without residue. The rise of temperature on the solution of a whole equivalent of the anhydrous salt, or 92·53 grains, was $1^\circ{\cdot}57$; on solution of one-half of an equivalent, or 46·2 grains, $0^\circ{\cdot}80$; which gives $1^\circ{\cdot}60$ for the whole equivalent.

Rise on solution of $Mg\,O, S\,O_3 + K\,O, S\,O_3$ . . .	$1^\circ{\cdot}60$
Fall on solution of $Mg\,O, S\,O_3 + K\,O, S\,O_3 + 6\,H\,O$	$2^\circ{\cdot}30$
Whole heat disengaged by $Mg\,O, S\,O_3 + K\,O, S\,O_3$	$3^\circ{\cdot}90$

The crystallized sulphate of magnesia and potash, dried by a nitre-bath, was found to retain 18·32 water to 100 anhy-

drous salt. Now 18·24 water represents 3 H O; the crystallized salt has consequently lost one-half of its water, retaining only three atoms. Of this salt, an equivalent quantity, or 109·4 grains, were dissolved in 1000 grains of water, with a fall in three experiments of 1°·35, 1°·30 and 1°·35, of which the mean is 1°·33.

Fall on solution of $MgO, SO_3 + KO, SO_3 + 3HO$ 1°·33.

The fall on the solution of this hydrate is less than on the solution of the former, by the heat disengaged in the combination of the salt with the deficient three atoms of water. The heat disengaged by the union of the salt with the first three atoms of water comes therefore to be 2°·93, and with the second three atoms of water 0°·97, making together 3°·90. Hence as nearly as possible three times as much heat are disengaged by the first three atoms of water as by the last three atoms.

4. *Sulphate of Magnesia and Ammonia.*—The solution of an equivalent quantity, 113·13 grains, of the crystallized salt in 1000 grains of water was attended by a fall of temperature, in two experiments, of 2°·20 and 2°·15, of which the mean is 2°·17. If dissolved in 976·2 grains of water, like the potash salt, the fall would have been about $\frac{1}{30}$th more, or 2°·24.

Fall on solution of $MgO, SO_3 + NH_4O, SO_3 + 6HO$ 2°·24.

The fall on the solution of the corresponding potash salt was 2°·30.

5. *Protosulphate of Iron and Ammonia.*—The solution of 122·17 grains, the equivalent quantity of the crystallized salt in 1000 grains of water, was attended with the same fall of 2°·20 in two experiments. But this determination should be increased by $\frac{1}{30}$th, like the last; the fall then becomes 2°·27.

Fall on solution of $FeO, SO_3 + KO, SO_3 + 6HO$. 2°·27.

6. *Sulphate of Manganese and Ammonia.*—In two experiments 61·20 grains of the crystallized salt, being one half of the equivalent quantity, were dissolved in 983 grains of water with a fall of 1°·11 and 1°·13; or 2°·24 for a whole equivalent.

Fall on solution of $MnO, SO_3 + NH_4O, SO_3 + 6HO$. 2°·24.

It thus appears that the heat of liquefaction of the four crystallized double salts, sulphate of magnesia and potash, sulphate of magnesia and ammonia, sulphate of iron and potash, and sulphate of manganese and ammonia, is sensibly the same.

7. *Sulphate of Zinc and Potash.*—The fall on the solution of half an equivalent, 69·26 grains, of the crystallized salt in 1000 grains of water, was in three experiments 1°·33, 1°·27 and 1°·30, of which the mean is 1°·30. The fall for a whole equivalent, 138·52 grains, is therefore 2°·60.

This salt was made anhydrous by a heat little short of redness, without being fused. Half an equivalent, 52·39 grains, was dissolved in 1000 grains of water with a rise of temperature of 0°·83 and 0°·87 in two experiments, of which the mean is 0°·85. The rise for a whole equivalent, 104·77 grains of the salt, is therefore 1°·70.

Rise on solution of $ZnO, SO_3 + KO, SO_3$	1°·70
Fall on solution of $ZnO, SO_3 + KO, SO_3 + 6HO$.	2°·60
Whole heat disengaged in the hydration of $ZnO, SO_3 + KO, SO_3$	4°·30

8. *Sulphate of Copper and Ammonia.*—Of the crystallized salt, 62·45 grains, or one half of the equivalent quantity, were dissolved in 983 grains of water with a fall of temperature of 1°·33 and 1°·28 in two experiments; giving 2°·63 for the whole equivalent.

Fall on solution of $CuO, SO_3 + NH_4O, SO_3 + 6HO$. 2°·63.

The fall on the solution of the two immediately preceding salts is therefore sensibly the same.

9. *Protosulphate of Iron and Potash.*—Of this salt in small but well-defined crystals, 67·66 grains, one half of the equivalent quantity, were dissolved in 983 grains of water with a fall, in two experiments, of 1°·25 and 1°·22; or for the whole equivalent 2°·47.

Fall on solution of $MnO, SO_3 + KO, SO_3 + 6HO$. 2°·47.

10. *Sulphate of Zinc and Ammonia.*—Of the crystallized salt half an equivalent, 62·64 grains, was dissolved in 983 grains of water with a fall of 1°·37 and 1°·36.

Fall on solution of $ZnO, SO_3 + KO, SO_3 + 6HO$ . . 2°·73.

11. *Sulphate of Copper and Potash.*—The solution of half an equivalent of the crystallized salt, 69·07 grains, in 1000 grains of water, was attended with a fall in two experiments of 1°·54 and 1°·50; or for the whole equivalent of the salt, 138·14 grains; the fall is 3°·08 and 3°·00, of which the mean is 3°·04.

The crystallized salt was made anhydrous by a heat short of redness, which had the effect of causing it to frit but did not fuse it. The solution of 52·2 grains, half an equivalent, in 1000 grains of water, was attended by a fall in two experiments of 1°·01 and 0°·96; or for a whole equivalent, 2°·02 and 1°·92, of which the mean is 1°·97.

Rise on solution of $CuO, SO_3 + KO, SO_3$	1°·97
Fall on solution of $CuO, SO_3 + KO, SO_3 + 6HO$.	3°·04
Whole heat disengaged in hydration of $CuO, SO_3 + KO, SO_3$	5°·01

The fall on the solution of the preceding crystallized double

salt is 3°·04, while the fall on the solution of its constituents dissolved separately is 1°·51 for the sulphate of potash, and 0°·67 for the hydrated sulphate of copper, making together 2°·18, which is less by 0°·86 than the former. The fall on the solution of the crystallized double sulphate of zinc and potash approaches more closely to the united falls of its constituents dissolved separately, the former being 2°·60 and the latter 1° + 1°·51 = 2°·51. The fall on the solution of the crystallized double sulphate of magnesia and potash is 2°·30; the united falls of its constituent salts 0°·92 + 1°·51 = 2°·43. No perceptible change of temperature was observed when the solutions of a pair of these salts are mixed to form the double salt; which is in accordance with the conclusion of Dr. Andrews, that no heat is evolved in the combination of salts.

I have not, however, succeeded in obtaining any direct proof of the formation of the double sulphates on mixture. To a solution of 77·97 grains, or one equivalent, of crystallized sulphate of copper in 1000 grains of water, 41·41 grains, one equivalent, of sulphate of ammonia dried at 234° were added and dissolved. The fall on the solution of the last salt was 0°·56, or the same as when the salt is dissolved in pure water. No change took place in the colour of the solution of the copper salt. The last salt was selected for this experiment because it appears more disposed to form double salts than even the sulphate of potash.

In certain cases, a double salt is formed on using a bisulphate, while it is not with the neutral sulphate; as in the formation of sulphate of zinc and soda, from sulphate of zinc and bisulphate of soda, but not from sulphate of zinc and neutral sulphate of soda. To a solution of 85·23 grains, or the equivalent, of crystallized bisulphate of potash in 1000 grains of water, 89·59 grains, or the equivalent, of crystallized sulphate of zinc were added and dissolved, with a fall of 1°·00, or the same as in pure water. To a similar solution of bisulphate of potash, 77·35 grains, or one equivalent, of crystallized sulphate of magnesia were added and dissolved, with a fall of 0°·86; the same fall also as on the solution of the latter salt in pure water. Yet the double salts crystallized out readily from both of these solutions.

I have formerly represented the anhydrous sulphate of magnesia and potash as corresponding with the protohydrated sulphate of magnesia. Now both these salts assume six atoms of water, and the heat then disengaged by the two salts is nearly the same:—

	Heat of hydration.
$Mg\,O, S\,O_3 + K\,O, S\,O_3$	3°·90
$Mg\,O, S\,O_3 + H\,O$	3°·95

When the corresponding salts of zinc are compared, the same equality is not observed, but other relations appear.

	Heat of hydration.
$ZnO, SO_3 + KO, SO_3$	$4^{\circ}{\cdot}30$
$ZnO, SO_3 + HO$	$3^{\circ}{\cdot}45$
ZnO, SO_3	$5^{\circ}{\cdot}17$

These quantities of heat and the quantity disengaged by the anhydrous sulphate of zinc, have a remarkable relation among themselves; if they be all divided by $0^{\circ}{\cdot}86$, we have

	Ratios of heat of hydration.
$ZnO, SO_3 + HO$	4·01
$ZnO, SO_3 + KO, SO_3$	5
ZnO, SO_3	6·01

The quantity of heat disengaged by the first atom of water on uniting with sulphate of zinc is $1^{\circ}{\cdot}71$. If it had been only half that quantity, or $0^{\circ}{\cdot}86$, and had the deficient $0^{\circ}{\cdot}86$ been evolved by the combination of the six following atoms, in addition to the heat they actually evolve, then the heat disengaged by the six atoms of water which unite with protohydrated sulphate of zinc, and by the double sulphate of zinc and potash, would be the same in both salts, as it is the same in the two corresponding salts of magnesia.

The heat evolved by the corresponding copper salts with their ratios, is as follows:—

	Heat of hydration.	Ratios.
$CuO, SO_3 + HO$ . . .	$2^{\circ}{\cdot}93$	4°
CuO, SO_3	$4^{\circ}{\cdot}40$	6°
$CuO, SO_3 + KO, SO_3$.	$5^{\circ}{\cdot}01$	$6^{\circ}{\cdot}86$

It is to be observed, however, that while the protohydrate of sulphate of copper combines with only four atoms of water, the sulphate of copper and potash combines with six atoms; the usual comparison cannot therefore be made between these two salts.

The principal numerical results of the paper are exhibited in the following tables:—

1. Heat absorbed by equivalent quantities of crystallized salts on dissolving in water.

Sulphate of magnesia	7 HO	$0^{\circ}{\cdot}92$
Sulphate of zinc	...	$1^{\circ}{\cdot}00$
Protosulphate of iron	...	$1^{\circ}{\cdot}06$
Sulphate of copper	5 HO	$0^{\circ}{\cdot}67$
Sulphate of manganese	...	$0^{\circ}{\cdot}12$
Sulphate of magnesia and potash .	6 HO	$2^{\circ}{\cdot}30$
Sulphate of magnesia and ammonia .	...	$2^{\circ}{\cdot}24$
Sulphate of manganese and ammonia	...	$2^{\circ}{\cdot}24$

Sulphate of iron and ammonia . .	6 H O	2°·27
Sulphate of iron and potash . . .	...	2°·47
Sulphate of zinc and potash . . .	...	2°·60
Sulphate of copper and ammonia .	...	2°·63
Sulphate of zinc and ammonia . .	...	2°·73
Sulphate of copper and potash . .	...	3°·04
Sulphate of soda	10 H O	4°·59
Sulphate of potash	anhydrous	1°·51
Sulphate of ammonia	...	0°·51
Chromate of potash	...	1°·18
Bichromate of potash	...	3°·96
Nitrate of potash	...	3°·96
Terchromate of potash	...	2°·28
Biphosphate of potash	2 H O	2°·24
Binarseniate of potash	...	2°·26
Sulphate of water and potash . . .	anhydrous	1°·95

2. Heat disengaged in the complete hydration of anhydrous salts.

Sulphate of magnesia	5°·25
Sulphate of zinc	5°·17
Sulphate of copper	4°·40
Sulphate of manganese	3°·34
Sulphate of magnesia and potash .	3°·90
Sulphate of zinc and potash . .	4°·30
Sulphate of copper and potash .	5°·01

3. Heat disengaged by the combination of the first atom of water in the magnesian sulphates.

Sulphate of water	1°·47
Sulphate of copper	1°·47
Sulphate of manganese	1°·43
Sulphate of magnesia	1°·30
Sulphate of zinc	1°·71

Simple relations are observed between the quantities of heat disengaged by the sulphates of magnesia and zinc, which appear to belong to one class, while the sulphates of water, copper and manganese belong to another class.

LVIII. *On the Invention of the Circular Parts.* *By Professor* De Morgan.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE object of this communication is to point out that a very material portion of Napier's "rule of circular parts" was published by Torporley twelve years before it was pub-

lished by Napier. I do not mean that Torporley gave his theorems in as elegant a form as Napier, nor do I deny that Napier abbreviated Torporley: what I say is that, of that abbreviation which is usually attributed entire to Napier, a considerable part belongs to Torporley. Nor am I by any means convinced that Napier had seen Torporley's work: but, according to the rule established in such cases, the *first* publisher must take precedence of all subsequent ones, whether independent discoverers or not.

Napier's circular parts are in his first *mathematical* publication, the *Mirifici logarithmorum Canonis descriptio*, 1614, which is most easily seen in the reprint by Baron Maseres (*Scriptores Logarithmici*, vol. vi.). The *circular parts* are in pp. 511, 512 of the last-named volume.

Torporley's work is *Diclides Coelometricæ, seu valvæ astronomicæ universales, omnia artis totius munera Psephophoretica in sat modicis finibus duarum Tabularum methodo nova, generali, et facilima continentes. Præeunte directionis accuratæ consumata Doctrina, Astrologis hactenus plurimùm desiderata. Authore Nath. Torporlæo Salopiensi in secessu Philothcoro.* London, 1602. An account of Torporley may be seen in Anthony Wood's *Athenæ Oxonienses*, or in the article *Vieta* in the Penny Cyclopædia. An account of his work is in Delambre's *Astronomie Moderne*, vol. ii. p. 36. It is very strange that Delambre should not have seen that the very description which he gives almost amounts to stating Napier's rules: but it is to be remembered that these circular parts, so celebrated in Britain, have hardly ever been used abroad. Delambre himself (Astronomy, vol. i. p. 205) says that he prefers to remember the six equations at once: a preference in which I heartily concur.

There never was, perhaps, a more ludicrous mnemonical attempt than that of Torporley. His rules are first *diclides*, they then become *valvæ*. These valves, six in number, then receive names: they are *Carcer*, *Hasta*, *Forfex*, *Siphon*, *Corvus*, and *Funda*. These six valves, namely the prison, the spear, the shears, the siphon, the crow (a pickaxe), and the sling, are then mounted on two *mitres*; Carcer, Siphon and Funda on one; Corvus, Hasta and Forfex on the other. This he calls *mitrosphærica memorabilis*. But we have not done with metaphor yet: for as soon as the valves, under their new names, are fairly established on the mitres, one of each set becomes the mother, and the other two the daughters. Here is the reduction of the six cases to two. Torporley then gives rules for the reduction of either daughter to the mother, and discovers the necessity for using the complements of the

data. He points out in the last chapter that the same formulæ will apply to all the cases of each triplicity, and his two formulæ resemble, of course, those of Napier in their structure. But Torporley has not accomplished the same amount either of symmetry or abbreviation which appears in the rules of Napier. The reduction of all the six cases to two, and the first exhibition of an organized mechanical mode of reducing each of the six cases to its primitive, belongs to him: Napier afterwards did the latter in a better manner, without the necessity of mnemonical verses.

Torporley has given two tables of double entry, which Delambre says are the most obscure and incommodious that ever were made. The first is neither one nor the other; a and b being the arguments, and c the tabulated result, it amounts to $\tan c = \tan a \times \sin b$, the double entry being contrived like that of the common multiplication table. Of the second table, as the book is scarce, I subjoin half-a-dozen instances.

	30°			
	G	G	M	M
	21		27	
24°		8		33
	12		54	

	54°			
	G	G	M	M
	27		55	
70°		26		5
	1		50	

	70°			
	G	G	M	M
	39		14	
54°		30		46
	8		28	

	90°			
	G	G	M	M
	71		58	
19°		18		2
	53		56	

	19°			
	G	G	M	M
	9		30	
90°		9		30
	0		0	

	147°			
	G	G	M	M
	88		3	
59°		58		57
	29		6	

As far as the formulæ for right-angled triangles are concerned, this table applies as follows. The sine of the angle on the left multiplied by the sine of the upper angle in the square compartment, gives the sine of the second angle in that compartment. Thus Torporley means to say that

$$\sin 24^\circ \times \sin 21^\circ 27' = \sin 8^\circ 33'.$$

Those who like such questions may find out the meaning of the other parts of the table.

The question whether Napier had seen Torporley's work cannot easily be settled: he uses the word *triplicity*, which is one of frequent occurrence in Torporley, and the figure of his demonstration contains the three triangles put together in exactly the same way as Torporley's *mother and daughters* come together on the *mitre*. These circumstances are not conclusive, but they are suspicious: *triplicitas* was by no

means a common word among mathematicians; it was a technical term of judicial astrology, and would probably be avoided by the geometer: Torporley was an astrologer, as appears from the opening of his work. *Trias* and *ternio* would suggest themselves first (*trinitas* being excluded for an obvious reason). On this word probably the question will turn: if it should be found that no mathematicians of the period use the word *triplicitas* except Torporley and Napier, it will be difficult to avoid presuming that the latter must have seen the work of the former.

I remain, Gentlemen,
Yours faithfully,

University College, March 13, 1843. A. De Morgan.

LIX. *Demonstration of some useful Theorems in the Geometry of Coordinates. By* William Rutherford, *Esq., F.R.A.S., Royal Military Academy**.

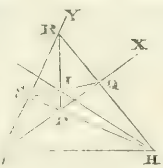
THEOREM I.—If the equations of two straight lines be

$$\frac{x}{\alpha}+\frac{y}{\beta}=1 \text{ and } \frac{x}{\alpha_1}+\frac{y}{\beta_1}=1,$$

then will the *sum* of these equations, viz.

$$\left\{\frac{1}{\alpha}+\frac{1}{\alpha_1}\right\}x+\left\{\frac{1}{\beta}+\frac{1}{\beta_1}\right\}y=2,$$

be the equation of the straight line passing through the point of intersection of these lines, and the point of intersection of the diagonals of the quadrilateral formed by the intersection of the given lines with the axes of coordinates.



Let O X, O Y be the axes of coordinates having any angle of ordination, and let P, Q, R, S be any four points in these coordinate axes; then if O P $=\alpha$, O Q $=\alpha_1$, O S $=\beta$, and O R $=\beta_1$, the equations of the lines S P and R Q, drawn through the points S, P and R, Q are respectively

$$\frac{x}{\alpha}+\frac{y}{\beta}=1 \text{ and } \frac{x}{\alpha_1}+\frac{y}{\beta_1}=1. \quad . \quad . \quad . \quad (1.)$$

Now these lines must either be parallel, or they will meet if produced. Let them meet when produced in H, and let I be the point of intersection of the diagonals S Q and R P of the quadrilateral P Q R S. Join O H, and through H and I

* Communicated by the Author.

draw the straight line H I. Then the equations of the diagonals P R and S Q are respectively

$$\frac{x}{\alpha} + \frac{y}{\beta_1} = 1 \text{ and } \frac{x}{\alpha_1} + \frac{y}{\beta} = 1; \quad . \quad . \quad . \quad (2.)$$

and since the point H is common to both the lines S P and R Q; and the point I common to both the diagonals S Q and P R; therefore the *sum* of the equations in (1.), viz.

$$\left\{\frac{1}{\alpha} + \frac{1}{\alpha_1}\right\} x + \left\{\frac{1}{\beta} + \frac{1}{\beta_1}\right\} y = 2, \quad . \quad . \quad (3.)$$

is evidently the equation of a line passing through H the point of intersection of the lines denoted by the equations (1.); but equation (3.) is also the sum of the equations (2.), and therefore equation (3.) is likewise the equation of a line passing through I the point of intersection of the diagonals of the quadrilateral P Q R S; hence the truth of the theorem is established.

If the lines S P and R Q are parallel, then the triangles O P S, O Q R will be equiangular, and therefore we have the relation

$$\frac{\alpha}{\alpha_1} = \frac{\beta}{\beta_1}, \text{ or } \beta_1 = \frac{\alpha_1}{\alpha}\beta;$$

and this value being substituted for β_1 in equation (3.), gives

$$\frac{\alpha_1 + \alpha}{2\alpha_1}\left\{\frac{x}{\alpha} + \frac{y}{\beta}\right\} = 1, \text{ or } \frac{x}{\alpha} + \frac{y}{\beta} = \frac{2\alpha_1}{\alpha_1 + \alpha}, \quad . \quad (4.)$$

which is the equation of the straight line parallel to the given line

$$\frac{x}{\alpha} + \frac{y}{\beta} = 1,$$

and passing through I the point of intersection of the diagonals of the trapezoid P Q R S.

Cor.—Hence it is obvious that the *difference* of the equations in (1.), viz.

$$\left\{\frac{1}{\alpha} - \frac{1}{\alpha_1}\right\} x + \left\{\frac{1}{\beta} - \frac{1}{\beta_1}\right\} y = 0, \quad . \quad . \quad (5.)$$

is the equation of the straight line O H passing through the origin of coordinates, and the point of intersection of the two lines S P and R Q. If these lines are parallel, then we have

$$\beta_1 = \frac{\alpha_1}{\alpha}\beta;$$

and by substituting this value of β_1 in equation (5.), we have

$$\frac{\alpha_1 - \alpha}{\alpha_1}\left\{\frac{x}{\alpha} + \frac{y}{\beta}\right\} = 0, \text{ or } \frac{x}{\alpha} + \frac{y}{\beta} = 0, \quad . \quad . \quad (6.)$$

which is the equation of the line passing through the origin, and parallel to the line whose equation is

$$\frac{x}{\alpha} + \frac{y}{\beta} = 1.$$

THEOREM II.—If the equations of two planes be represented by

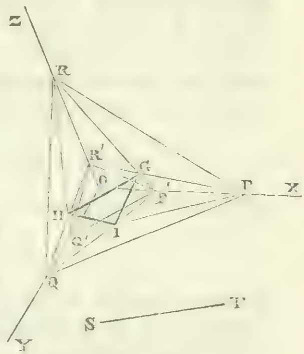
$$\frac{x}{\alpha} + \frac{y}{\beta} + \frac{z}{\gamma} = 1 \text{ and } \frac{x}{\alpha_1} + \frac{y}{\beta_1} + \frac{z}{\gamma_1} = 1,$$

then will the *sum* of these equations, viz.

$$\left\{\frac{1}{\alpha} + \frac{1}{\alpha_1}\right\} x + \left\{\frac{1}{\beta} + \frac{1}{\beta_1}\right\} y + \left\{\frac{1}{\gamma} + \frac{1}{\gamma_1}\right\} z = 2,$$

be the equation of the plane passing through the line of intersection of these planes, and through the three points of intersection of the diagonals of the three quadrilaterals formed by the intersection of the given planes with the axes of coordinates.

Let OX, OY, OZ be the oblique axes of coordinates, and let P, Q, R be any three points in the axes of x, y, z respectively, and P′, Q′, R′ any other three points in the same axes. Draw the traces P Q, Q R, R P, P′ Q′, Q′ R′, R′ P′ forming with the coordinate axes the three quadrilaterals P Q Q′ P′, Q R R′ Q′, P R R′ P′, and let I, H, G be the points of intersection of the diagonals of these quadrilaterals respectively. Then if we put



$$\begin{array}{l|l|l} OP = \alpha & OQ = \beta & OR = \gamma \\ OP' = \alpha_1 & OQ' = \beta_1 & OR' = \gamma_1, \end{array}$$

we shall have the equations of the several planes as below.

$$(P\,Q\,R) \quad . \quad . \quad \frac{x}{\alpha} + \frac{y}{\beta} + \frac{z}{\gamma} = 1 \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

$$(P'\,Q\,R) \quad . \quad . \quad \frac{x}{\alpha_1} + \frac{y}{\beta} + \frac{z}{\gamma} = 1 \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

$$(P\,Q'\,R) \quad . \quad . \quad \frac{x}{\alpha} + \frac{y}{\beta_1} + \frac{z}{\gamma} = 1 \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

$$(P\,Q\,R') \quad . \quad . \quad \frac{x}{\alpha} + \frac{y}{\beta} + \frac{z}{\gamma_1} = 1 \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

$$(P'\,Q'\,R') \quad . \quad . \quad \frac{x}{\alpha_1} + \frac{y}{\beta_1} + \frac{z}{\gamma_1} = 1 \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

$$(P\,Q'\,R') \quad . \quad . \quad \frac{x}{\alpha} + \frac{y}{\beta_1} + \frac{z}{\gamma_1} = 1 \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

$$(\mathrm{P}'\mathrm{Q}\,\mathrm{R}') \quad . \quad . \quad \frac{x}{\alpha_1}+\frac{y}{\beta}+\frac{z}{\gamma_1}=1 \quad . \quad . \quad . \quad . \quad (7.)$$

$$(\mathrm{P}'\mathrm{Q}'\mathrm{R}) \quad . \quad . \quad \frac{x}{\alpha_1}+\frac{y}{\beta_1}+\frac{z}{\gamma}=1 \quad . \quad . \quad . \quad . \quad (8.)$$

Now if we take the *sum* of the equations (1.) and (5.), (2.) and (6.), (3.) and (7.), (4.) and (8.), we shall, in each case, have the same resulting equation, viz.

$$\left\{\frac{1}{\alpha}+\frac{1}{\alpha_1}\right\}x+\left\{\frac{1}{\beta}+\frac{1}{\beta_1}\right\}y+\left\{\frac{1}{\gamma}+\frac{1}{\gamma_1}\right\}z=2. \quad (9.)$$

Let S T be the line of intersection of the two planes in (1.) and (5.); that is of the planes P Q R and P′Q′R′; then the line S T being common to the planes P Q R and P′Q′R′, and since the point I is common to the planes P′Q R and P Q′R′, the point H to the planes P Q′R and P′Q R′, and the point G to the planes P Q R′ and P′Q′R; therefore it is obvious that equation (9.), which is the sum of the equations of these planes, taken two and two, is the equation of the plane which passes through the line S T and through the three points G, H, I the points of intersection of the diagonals of the quadrilaterals formed by the two planes P Q R and P′Q′R′ with the axes of coordinates.

Cor.—If the equations of two planes be denoted by

$$\frac{x}{\alpha}+\frac{y}{\beta}+\frac{z}{\gamma}=1 \text{ and } \frac{x}{\alpha_1}+\frac{y}{\beta_1}+\frac{z}{\gamma_1}=1,$$

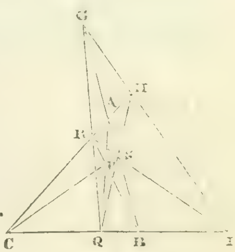
then will the *difference* of these equations, viz.

$$\left\{\frac{1}{\alpha}-\frac{1}{\alpha_1}\right\}x+\left\{\frac{1}{\beta}-\frac{1}{\beta_1}\right\}y+\left\{\frac{1}{\gamma}-\frac{1}{\gamma_1}\right\}z=0,$$

be the equation of the plane which passes through the line of intersection of these planes, and the origin of coordinates.

The principles developed in these theorems and corollaries are very effective in analytical inquiries, and in order to point out their application I shall add the following demonstration of a well-known theorem.

Theorem.—If straight lines A Q, B R, C S be drawn from the angles A, B, C of a triangle through any point P to meet the opposite sides in Q, R, S; and if Q R, Q S, R S be drawn to meet the sides of the triangle in G, H, I; then will the points G, H, I be in the same straight line.



Take H C, H Q for the axes of x and y respectively, and put H A $=\alpha_1$, H R $=\alpha_2$, H C $=\alpha_3$, H S $=\beta_1$, and H Q $=\beta_2$; then we have the equations of the several lines as below.

$$(\mathrm{A\,B}) \ldots \frac{x}{\alpha_1} + \frac{y}{\beta_1} = 1 \ldots (1.) \qquad (\mathrm{R\,S}) \ldots \frac{x}{\alpha_2} + \frac{y}{\beta_1} = 1 \ldots (4.)$$

$$(\mathrm{G\,Q}) \ldots \frac{x}{\alpha_2} + \frac{y}{\beta_2} = 1 \ldots (2.) \qquad (\mathrm{A\,Q}) \ldots \frac{x}{\alpha_1} + \frac{y}{\beta_2} = 1 \ldots (5.)$$

$$(\mathrm{B\,C}) \ldots \frac{x}{\alpha_3} + \frac{y}{\beta_2} = 1 \ldots (3.) \qquad (\mathrm{C\,S}) \ldots \frac{x}{\alpha_3} + \frac{y}{\beta_1} = 1 \ldots (6.)$$

Now (Theorem I., *Cor.*) if we subtract (4.) and (1.) from (3.) and (2.) respectively, we shall have the equations of H I and H G, viz.—

$$(\mathrm{H\,I}) \quad \ldots \quad \left\{\frac{1}{\alpha_3} - \frac{1}{\alpha_2}\right\} x + \left\{\frac{1}{\beta_2} - \frac{1}{\beta_1}\right\} y = 0 \quad \ldots \quad (7.)$$

$$(\mathrm{H\,G}) \quad \ldots \quad \left\{\frac{1}{\alpha_2} - \frac{1}{\alpha_1}\right\} x + \left\{\frac{1}{\beta_2} - \frac{1}{\beta_1}\right\} y = 0 \ldots \quad \ldots \quad (8.)$$

These equations will be identical if it can be shown that the coefficients of x are equal, and to effect this, the condition that the lines A Q, B R, C S all pass through the same point P must be employed. Hence if we add the equations of C Q and A S, viz. (3.) and (1.), we shall have the equation of B P (Theorem I.), viz.

$$\left\{\frac{1}{\alpha_1} + \frac{1}{\alpha_3}\right\} x + \left\{\frac{1}{\beta_1} + \frac{1}{\beta_2}\right\} y = 2 \ldots \quad . \quad . \quad (9.)$$

To find where this line cuts the axis of x, make $y = 0$, and we get

$$x = \frac{2}{\dfrac{1}{\alpha_1} + \dfrac{1}{\alpha_3}};$$

and this must evidently be the value of H R, because B P R is a *straight* line by hypothesis; but H R $= \alpha_2$, and therefore we must have

$$\frac{2}{\dfrac{1}{\alpha_1} + \dfrac{1}{\alpha_3}} = \alpha_2, \text{ or } \frac{2}{\alpha_2} = \frac{1}{\alpha_1} + \frac{1}{\alpha_3},$$

$$\therefore \quad \frac{1}{\alpha_2} - \frac{1}{\alpha_1} = \frac{1}{\alpha_3} - \frac{1}{\alpha_2}, \text{ by transposition;}$$

and consequently equations (7.) and (8.) are identical, and therefore the points G, H, I range in the same straight line.

The form of the equation of a straight line used in these inquiries is well adapted to the demonstration of a class of theorems concerning the intersections of straight lines, of which the preceding example is an instance, and a variety of others will be found in the forthcoming second volume of Hutton's Course, by my talented colleague Mr. Davies,

where the principle developed in the first of these Theorems is frequently applied with much simplicity, elegance and advantage. I am not without a hope that the publication of that volume will materially contribute to the improvement of the taste of the young geometer, not only from the great number of original and well-chosen discussions which are introduced into it, but also from the many beautiful investigations with which it is enriched.

I have also to add that my attention was called to the principle of combining the equations of straight lines as here employed by my colleague Mr. Fenwick, and that some further inquiries of this kind will appear in a tract which I am, in conjunction with that gentleman, about to publish, and which may possibly be succeeded by others of a similar nature.

LX. *Remarks on the Rev.* B. Bronwin's *paper on* M. Jacobi's *Theory of Elliptic Functions. By* A. Cayley, *Esq., B.A., F.C.P.S., Fellow of Trinity College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

ALLOW me to insert in your Magazine a few remarks on a paper "On M. Jacobi's Theory of Elliptic Functions," which appeared in your last Number, in which the author, Mr. Bronwin, attempts to show that some of M. Jacobi's formulæ are erroneous. As far as I can understand his argument, he wishes to deduce from the equation

$$\mathrm{s}\,a\frac{u}{\mathrm{M}} = \mathrm{C}\,\mathrm{s}\,a\,u\,\mathrm{s}\,a\,(u+2\,\omega)\ldots\mathrm{s}\,a\,(u+2\,(n-1)\,\omega)$$

(numbered (3.) in the paper referred to) the conclusion that C is necessarily, in all cases in which the formula exists at all, given by the equation

$$\frac{1}{\mathrm{C}} = \pm\,\mathrm{s}\,a\,\omega\,\mathrm{s}\,a\,3\,\omega\ldots\ldots\ldots\mathrm{s}\,a\,(2\,n-1)\,\omega.$$

Omitting for the present his remarks upon the form $\omega = \frac{2\,r\,\mathrm{K} + 2\,r'\,\mathrm{K}'\sqrt{-1}}{n}$, for the three remaining forms of ω,

$$\text{viz. } \omega = \frac{\overline{2\,r+1}\,\mathrm{K} + 2\,r'\,\mathrm{K}'\sqrt{-1}}{n},$$

$$\omega = \frac{2\,r\,\mathrm{K} + \overline{2\,r'+1}\,\mathrm{K}'\sqrt{-1}}{n},$$

$$\omega = \frac{\overline{2\,r+1}\,\mathrm{K} + \overline{2\,r'+1}\,\mathrm{K}'\sqrt{-1}}{n},$$

he says, "For these forms the equations (3.) and (4.)* are in their simplest forms; their second members vanish when $u = 0$, 2ω, 4ω, &c., but never between these values. Consequently, while u increases by 2ω, $\frac{u}{M}$ increases by $2H$, neither more nor less, if $\frac{u}{M} = H$ when its amplitude is $\frac{\pi}{2}$. Whilst, therefore, u from 0 becomes ω, $\frac{u}{M}$ from 0 becomes H. Let them have these values in (3.), and we obtain $s\,a\,H = 1 = \pm\, C\, s\,a\,\omega\, s\,a\,3\,\omega \ldots s\,a\,(2n-1)\,\omega$, $\therefore \frac{1}{C} = \pm\, s\,a\,\omega \ldots s\,a\,(2\,n - 1)\,\omega$. This then is the general form of $\frac{1}{C}$, and M. Jacobi's denominator cannot be true, except in those cases in which it is reducible to it." I have quoted this verbatim, or I should probably have misrepresented it, for I am utterly unable to see the force of it. For anything I can see to the contrary, when u increases by $2\,\omega$, $\frac{u}{M}$ might increase by $2\,p\,H + 2\,p'\,H' \sqrt{-1}$ (as Mr. Bronwin expresses it), or ω, instead of being equal to M H, might be equal to $M\,(p\,H + p'\,H' \sqrt{-1})$, p, p' being integers to be determined. Hence when u from 0 becomes ω, $\frac{u}{M}$ from 0 becomes $p\,H + p'\,H' \sqrt{-1}$. Substituting in (3.) the first side is of one of the forms $\pm\, 1$, 0, $\pm\, \infty \sqrt{-1}$, $\pm\, \frac{1}{(k)}$, according to the forms of p and p'. The second side contains the factor $s\,a\,n\,\omega$, which is likewise of one of the forms $\pm\, 1$, 0, $\pm\, \infty \sqrt{-1}$, $\pm\, \frac{1}{(k)}$, according to the form of ω. Thus it may happen that C, instead of having Mr. Bronwin's value, is given by an equation $C = \frac{0}{0}$, or $C = \frac{\infty}{\infty}$, or that its value cannot be determined by this process, and *may*, on the contrary, be determined by M. Jacobi's formula. In the remaining case excepted from Mr. Bronwin's reasoning, he certainly shows that M. Jacobi's formulæ are not in their simplest form, but by no means that they fail; on the contrary he rather confirms them. I may just mention the argument of a note in the Cambridge Mathematical Journal,

* I have not written down here the equation (4.), which is not necessary for my present purpose.

of which, notwithstanding the utter contempt Mr. Bronwin expresses for it, I must confess myself to be the author. The object of it was to show that the formulæ

$$\mathrm{s}\,a\,v = \frac{\mathrm{s}\,a\,u\,\mathrm{s}\,a\,(u+2\,\omega)\ldots\ldots\mathrm{s}\,a\,(u+2\,\overline{n-1}\,\omega)}{\mathrm{s}\,a\,\omega\,\mathrm{s}\,a\,3\,\omega\ldots\ldots\mathrm{s}\,a\,(2\,n-1)\,\omega},$$

$$\mathrm{c}\,a\,v = \frac{\mathrm{c}\,a\,u\,\mathrm{c}\,a\,(u+2\,\omega)\ldots\ldots\mathrm{c}\,a\,u+2\,\overline{n-1}\,\omega}{\mathrm{c}\,a\,2\,\omega\,\mathrm{c}\,a\,4\,\omega\ldots\ldots\,\mathrm{c}\,a\,2\,n-2\,\omega},$$

for the forms of ω objected to by Mr. Bronwin, were absolutely inconsistent. Suppose in the second of these $u=\omega$, $\mathrm{c}\,a\,v$ only vanishes in the particular case $\omega = \frac{2\,\overline{r+1}\,\mathrm{K} + 2\,r'\,\mathrm{K}'\sqrt{-1}}{n}$; hence it is only in this case that the first formulæ can coexist with the second. This is in substance the note in question, only I was guilty of an oversight not affecting the argument, which Mr. Bronwin has very correctly pointed out. I am at present not at all certain that he would assert that the above two formulæ can coexist; but as the second is one of Jacobi's, and his objections are founded upon the fact of one of the factors of the first becoming infinite or zero, for some of the forms of ω, I do not see how they apply, except on the supposition of the first formula being deducible from the second. In fact Mr. Bronwin has nowhere brought any objection against any particular step of M. Jacobi's reasoning, and it will be seen on examination that there is none to be brought. I must still remain of the opinion that he has only proved that his own formulæ do fail when he says they fail, and that M. Jacobi's are perfectly correct in every case.

I remain,

Yours respectfully,

Regent's Park, April 13, 1843. A. CAYLEY.

LXI. *On a new System of inactive Tithonographic Spaces in the Solar Spectrum analogous to the Fixed Lines of* Fraunhofer. *By* JOHN WILLIAM DRAPER, *M.D., Professor of Chemistry in the University of New York**.

[Illustrated by Plate III.]

1. WHEN a beam of the sun's light, directed horizontally by a heliostat, is thrown into a dark room, and passing through a chink with parallel sides, is received on the surface of a homogeneous flint glass prism, which refracts it at the angle of minimum deviation, and after its passage

* Communicated by the Author.

through the prism is converged to a focal image on a white screen by the action of an achromatic lens, the spectrum which results is given in great purity, and Fraunhofer's lines are quite apparent. The larger ones are seen by the most casual inspection.

2. A tithonographic surface, after being placed in this spectrum, exhibits impressions of an analogous character, being covered with the representations of multitudes of inactive lines varying greatly in dimensions.

3. After several attempts last summer I succeeded in discovering these lines, and have obtained impressions of them sufficiently perfect.

4. Before proceeding to the description of the mode which is to be followed, and of the characters of the lines themselves, I cannot avoiding calling attention to the remarkable circumstance which has frequently presented itself to me of a great change in the *relative visibility* of Fraunhofer's lines, when seen at different periods. There are times at which the strong lines seen in a red ray are so feeble that the eye can barely catch them, and then again they come out as dark as though marked in India ink on the paper. During these changes the other lines may or may not undergo corresponding variations. The same observation equally applies to the blue and yellow rays. It has seemed to me that the lines in the red are more visible as the sun approaches the horizon, and those at the more refrangible end of the spectrum are obvious in the middle of the day.

5. A beam of the sun, passing horizontally from a heliostat mirror into a dark room, was received on a screen with a slit in its centre, the slit being formed by a pair of parallel knife edges, one of which was moveable by a micrometer screw; the instrument being in fact the common instrument used for showing diffracted fringes. The screw was adjusted so as to give an aperture $\frac{1}{52}$ inch wide, and the light passing through fell upon an equiangular flint-glass prism placed at the distance of eleven feet. Immediately on the posterior face of the prism, the ray was received on an achromatic lens, the object-glass of a telescope, and brought to a focus at a distance of six feet six inches, at which place an arrangement was adjusted for exposing white paper screens, on which the more prominent fixed lines might be seen and their position marked, or sensitive plates substituted for the screens, occupying precisely the same position. The lines on the screens could therefore be compared with those on the sensitive surfaces as to position and magnitude with considerable accuracy. In these trials I have generally used an achromatic lens, but the lines can be

beautifully seen by employing a common double convex if the screen be inclined forward in the way described in this Journal for December 1842. Either way answers very well.

6. In order to identify these lines I have made use of the map of the spectrum published by Prof. Powell in the Report of the British Association for 1839. With the instrumental arrangement described they are exceedingly distinct, and no difficulty arises in the identification of the more prominent ones. The spectrum, with which I have worked, occupies upon the screen a space of nearly four inches and a quarter in length from the red to the violet, or, more correctly speaking, from the ray marked in that map A to the one marked *k*. In stating, however, that no difficulty arises in identifying these lines, I ought to add that I am referring to that particular map. In the figure annexed to Sir J. Herschel's treatise on Light, in the *Encyclopedia Metropolitana*, the ray marked G seems to differ from that of the Report. But Prof. Powell's map being drawn from his personal observations, and with reference to these very difficulties, as it coincides with my own observations and measures, I have employed it and therefore take the letters he gives.

7. It will be understood that the *whole* spectrum and *all* its lines cannot be obtained at one impression. The difficulty which is in the way of effecting this rests in the circumstance, that different regions of the spectrum act with different power in producing the proper effect. Thus, if on common yellow iodide of silver the attempt were made to procure all the lines at one trial, it would be found that the blue region would have passed to a state of high solarization, and all its fine lines become extinguished by being overdone long before any well-marked action could be traced at the less refrangible extremity. We have therefore to examine the different regions in succession, exposing the sensitive surface to each for a suitable length of time.

8. In the Plate which accompanies this paper [Plate III.], I have given on the left side a representation of the larger lines of Fraunhofer, the letters being derived, as has been said, from Prof. Powell's map. The position of the lines is, however, copied from my own spectrum as closely as I have been able to accomplish it.

9. In order that a comparison may be made between the new system of lines and those of Fraunhofer, the right side of the Plate gives a tithonographic representation of them as obtained on a Daguerreotype plate which has been iodized to the yellow, brought by the vapour of bromine to the red, and then slightly exposed to the vapour of chloride of iodine. The

map is so adjusted, in the plate, as to have its lines by the side of those of Fraunhofer which have the same name. Referring, therefore, to the Plate, it will be seen that there are beyond the red ray three extra-spectral lines which I have marked α, β, γ. These, however, I have only occasionally found, for from the general diminution of effect in that region they do not always come out in a plain and striking manner. None of Fraunhofer's lines in the yellow and green are given, but G and its companions are very strongly marked, as is also the group about *i*. But by far the most striking in the whole tithonograph are those marked H and *k*; and now, passing beyond the violet, and out of the visible limits of the spectrum, four very striking groups make their appearance. The first line of each of these groups I have marked in continuation of Fraunhofer's nomenclature, M, N, O, P. In L there are three lines, in M five, in N three, in O three, and in P five.

10. Besides these larger groups the whole tithonograph is crossed by hundreds of minuter ones, so that it is utterly impossible to count them. If, as it has been said, nearly 600 have been counted between A and H, I should think there must be quite as many between H and P. In speaking, therefore, of these lines as though they were strong individual ones, the expression is to be taken with some limitation. It is quite likely that each of those bolder lines is made up of a great number that are excessively narrow and close together.

11. If the absorptive action of the sun's atmosphere be the cause of this phænomenon, that action takes place much more powerfully on the more refrangible and extra-spectral region. The lines exhibited there are bold and strongly developed; they are crowded in groups together.

12. I cannot doubt, judging from analogy, that, by proper modes of investigation, similar lines might be detected in the calorific extra-spectral region.

13. The contrast between the visible and tithonographic spectra is maintained by the non-appearance of lines in the yellow and green regions. Once only I thought I perceived a line corresponding to Fraunhofer's F, but it was exceedingly faint and on the whole doubtful.

14. Fraunhofer's lines which occur on the orange, yellow, and green spaces, thus leaving no corresponding impression, another argument is furnished of the independence of the tithonic and luminous rays. It is probable that more perfect arrangements than I have used would give the *whole* spectrum as though it were full of these inactive spaces, and in stating that nothing like Fraunhofer's lines exist in those medial regions, I therefore simply wish it to be understood that I can

find nothing at all corresponding in magnitude to the great lines marked D, E, F, though hundreds of microscopic ones may probably exist in these very spaces.

15. The position of the lines as represented on the sensitive surface, is found to be, as might have been anticipated, independent of the chemical nature of that surface. The iodide of silver gives them in the same places as the bromide.

16. An argument might be drawn, as has been said, from the absence of these lines in the yellow and green spaces, as to the independence of the dark rays and light. This is, however, only another proof of a fact of which we have now abundant evidence. In 1834, when my attention was first fixed forcibly on these things, and I began to make prismatic analyses by the aid of sensitive paper, some of my first trials were directed to the detection of these fixed lines. At that time I was employing sensitive paper made with the bromide of silver, precisely as has been subsequently done in Europe; a number of the results were published in the American Journals during the year 1837. In the detection of these lines I failed entirely, but the bromuretted paper enabled me at that early period (whilst the attention of no other chemist was as yet turned to these matters) to trace the blackening action from far beyond the confines of the violet down almost to the other end of the spectrum. I distinctly made out that the dark rays underwent interference after the manner of their luminous companions, a result originally due to Arago, and printed some long papers in proof of the physical independence of the chemical rays, and light, and heat, throughout the spectrum.

17. These papers, which I shall probably republish this summer, may be found in the Journal of the Franklin Institute for 1837. On referring to them, it will be perceived, from the great number of remarkable typographical errors, that they have been printed from uncorrected proof-sheets. I resided at that time a great distance from Philadelphia, in which city they were published, and never saw them until long after they were in print.

18. The plates which accompany those papers will show that the process I then employed was the same as that described in this Journal (December 1842), that is to say, by passing the ray through absorbent media and then decomposing it by a prism. Six years have now elapsed since those experiments were published.

LXII. *On the Tithonotype, or Art of multiplying Daguerreotypes. By* John William Draper, *M.D., &c.**

1. In a paper "On the Action of the Rays of the Solar Spectrum on the Daguerreotype Plate," inserted in this Journal for the month of February 1843, which has just reached me, Sir John Herschel points out that a connexion may be traced between the phænomena of coloration impressed by the spectrum, and those of Newton's rings. With striking ingenuity he shows how a succession of positive and negative pictures may arise by prolonged solar action, and those shades of colour which the iodide of silver exhibits, under variable exposure to light, originate.

2. This hypothesis, however, as that able philosopher proceeds to state, is not unattended with difficulties, and after pointing out what those difficulties are, he shows how nevertheless it can account for an extensive group of facts. I regret that these difficulties are in the way, and that there are also other facts which appear to exclude the theory of thin plates from these phænomena.

3. *The Daguerreotype image in all its forms may be transferred by any copying process to other suitable surfaces. In other words it may be printed from.*

4. Sir D. Brewster was the first to show that the colours of mother-of-pearl might be impressed on any yielding surface. In the same manner so can the Daguerreotype image.

5. This is unquestionably the most important fact yet known in the history of these mysterious images, both in a theoretical and in a practical point of view. In a theoretical point of view, it shows us that it is among the phænomena of grooved, or striated, or dotted surfaces that the Daguerreotype is to be ranged, and in a practical point of view it shows the true mode of solving the great problem of producing from a given proof a multitude of copies.

6. In this Journal for September 1841 (p. 202. (37)), in speaking of the action of isinglass dried on the surface of the Daguerreotype pictures, I stated that I had succeeded with a process for multiplying copies, and promised on a future occasion to make it known: that promise I now proceed to redeem.

7. On referring to the paper in question the reader will perceive that the following facts are stated (p. 199 (20.)), that gum-arabic mucilage, dried on a common Daguerreotype, splits up bringing with it the white portions: that Russian

* Communicated by the Author.

isinglass (p. 200 (27.) p. 201 (34.)) dried in a similar manner, does the same thing, and will even rend off the yellow coating of iodine if it has not been previously removed.

8. Now, in addition, I have to state that if on a picture that has been fixed by a film of gold, so as to be irremoveable, a layer of isinglass be caused to dry and split up, it will bear on its surface a complete impression of the drawing, all the details being given with inexpressible beauty, the minutest lines and dots being present.

9. From the same plate a series of these impressions may be taken. The images that are on them may be seen either by reflected or transmitted light, in the former instance most favourably by placing them on black velvet.

10. I have hopes of improving this method so as to introduce it into effectual use. The practical difficulties that are in the way rest in the circumstance that the isinglass often splits off in chips instead of separating in one unbroken sheet. And the plate from which the impressions are taken, or with which the printing process is carrying on, becomes injured; not by having its surface removed, but by the isinglass adhering in circumscribed places, and obstinately refusing to detach itself.

11. This refinement on the art of printing, or rather of casting, might be supposed to give rise to very perishable results. This however is far from the case; I have now by me proofs made nearly two years ago, and they do not seem to have undergone any change. They have lain loosely in a drawer.

12. I presume, therefore, that any process which can exhibit the colours of mother-of-pearl will also exhibit Daguerreotype images. This lays open a variety of new branches of the photographic art.

13. As a name for these processes of copying the surface of a Daguerreotype, I would suggest the word TITHONOTYPE.

14. To carry this process into effect the operator proceeds as follows:—The Daguerreotype, which he designs to copy, is to be covered with a thin film of gold in the usual way, care being taken that the film is neither too thick nor too thin. If it be too thick the resulting copy is injured, and difficulties are more liable to arise in effecting the separation of the gelatinous coat; if too thin, the plate itself will suffer injury by having the figure torn off.

15. A clear solution of isinglass is next to be prepared; it must be of such a consistency that a drop of it poured on a cold metallic plate will speedily set. Much of the success of the process depends on this solution being properly made.

There is a substance in the market, which goes under the name of Cooper's Isinglass, which I have found much better than any other for these purposes.

16. The plate is to be arranged horizontally, with its face upwards, on some proper support, in the current of hot air that rises from a stove. The isinglass is to be poured on until a stratum about $\frac{1}{6}$th of an inch deep is upon the plate. It is then suffered to dry, the process being conducted so as to occupy two or three hours. When perfectly successful, as soon as the drying is complete, the film of isinglass now indurated into a tithonotype splits off, and on being examined either by reflected or transmitted light will be found to bear a minute copy of the original.

17. To return for a while to the theory of these images. Whilst thus it is plain that the optical effect depends on surface configuration alone, and does not seem to have any immediate relation to the thickness or thinness of a film, it is very different with the chemical effect on which the whole phænomenon depends.

18. *The Daguerreotype film, which has been under the influence of light, is polarized throughout its structure previous to mercurialization.*

19. I use the word "polarized" in its chemical sense. An illustration will serve to show the signification I attach to the term. When water is placed between platina electrodes its oxygen is liberated from one of them, and its hydrogen from the other, and the intervening liquid assumes a polar state, a series of decompositions and combinations going on. As that water is polarized, and undergoes polar decomposition, so too do the same phænomena hold in the case of the Daguerreotype film.

1st. We know that no iodine is ever evolved from the plate, even under the most prolonged action of the light. (Phil. Mag. Sept. 1841, p. 201.)

2nd. The cause of the final appearance of the image is due to silver being liberated on the anterior face of the plate. (Sept. 1841, p. 201.)

3rd. When, by the action of gelatine, the iodine and mercury are both removed from the plate, it is obvious that the plate has been corroded wherever the light fell. Iodine therefore has been evolved on the posterior face of the film, and is the cause of this corrosion.

20. From the circumstance, therefore, that iodine is evolved at the back of the film and silver at its front, and the film itself remaining the same in thickness throughout, it is obvious that there is a strong resemblance between this phænomenon and

that of the polar decomposition of water. The electro-positive and electro-negative elements are yielded up on opposite faces of the film, and its interior undergoes incessant polar changes,—the oppositely electric particles sliding as it were on one another.

21. In a late Number of this Journal I have described the remarkable power of certain electro-negative gases in operating the rapid detithonization of surfaces that have been changed by light. Since that paper was sent to England I perceive from the "Scientific Memoirs," that Professor Moser has published results of a similar kind. The true explanation of them appears to me to be very different from that which he gives; for his idea of vapours containing latent rays of particular orders of refrangibility or colour, rests on a very feeble analogy, and strikes me as entirely without support.

22. The view which I have taken of these phænomena, and to which allusion was made in the paper referred to, can be easily understood from what has just been said. The film, on a Daguerreotype plate, which has been disturbed by the tithonic rays but not yet mercurialized, is in a *polar condition of force*, its iodine is ready to unite with a new layer of silver behind, its silver is ready to be evolved in front. If it be exposed to mercurial vapours union at once takes place on that front face, and an amalgam is formed; if to the vapours of iodine or chlorine or bromine, an iodide, chloride or bromide of silver is formed. In an instant, its disturbing affinities being satisfied, the film reverts back to its former condition of equilibrium, and is precisely in the condition it was in before exposure to the light.

University, New York, March 7, 1843.

LXIII. *Facts relating to the Corpuscles of Mammiferous Blood, communicated to the Royal Society. By* Martin Barry, *M.D., F.R.SS. L. and E.**

NO observer can learn the structure of the blood-corpuscles, who does not carefully investigate their mode of origin, and patiently follow them through all their changes. Where are these changes to be seen? Not in blood taken from large vessels, which are merely channels for conveying it, but in that contained, and almost at rest, in the capillaries,—and especially in the capillary plexuses and dilatations; a remark which I believe is new, though many figures published by myself in the Philosophical Transactions show the observations on which it is grounded to have been long since made. But there is another source from which my information has been

* Communicated by the Author.

obtained—the large cells in the ovum. From these the corpuscles of the blood seem to have descended; and they undergo changes essentially the same.

1. The mammiferous blood-corpuscle, like one of the cells of the ovum, is at first a disc, or what is now called a "cytoblast," *i. e.* a cell-germ. It is not a flattened vesicle or cell. Like other discs or cytoblasts, however, it may and does *become* a cell; but then it is no longer flat. In the blood-disc you see a central, colourless, concave portion, around which lies the red colouring matter.

2. As usually met with, the blood-disc is *round*, with the exception of two or three instances in which, from the observations of Mandl in France and Gulliver in this country, it has been discovered to be *elliptical*. I have since found that even in Mammals where the blood-disc is usually met with round, *its original form is elliptical*. I have seen this to be the original form of the blood-disc in Man.

3. The discs first become round, continuing flat; subsequently they pass into an orange-shape, and lastly become globular. They also very much increase in size.

4. Along with these alterations in the form and size of the blood-discs, there takes place another change. Instead of a mere concavity, there is now seen a colourless, pellucid, semi-fluid substance; which, as the corpuscle becomes orange-shaped, is found to be, not in the centre, but *on one side*. It is the nucleus of the corpuscle—the corpuscle itself having become a cell. This pellucid substance or nucleus divides into and gives off globules. Each globule, appropriating to itself new matter, becomes a disc; and each disc, undergoing changes like the first, gives origin to other discs, a group of which constitutes the colourless corpuscle of the blood: for, with the changes now mentioned, the red colouring matter is consumed. Thus, as the red pass into the colourless corpuscles, there must exist all intermediate stages; between them no line of distinction can be drawn*.

5. The corpuscles of the blood are propagated by means of parent cells. A parent cell has its origin in a colourless corpuscle; this colourless corpuscle being an altered disc.

* The colourless corpuscles in other Vertebrata, for instance the Batrachians, being much smaller than their red corpuscles, cannot be these red corpuscles in an altered state. Nor is any such change to be expected here. The red corpuscles usually seen circulating in these animals are not, as in Mammalia, *discs*, but *nucleated cells*. Some of these nucleated cells, however, give origin to discs having very much the same form, size and general appearance as the blood-discs of the mammalia. In the Frog I saw such *discs* passing into the state of colourless globules, which, acted on by acetic acid, presented just the same appearance as the colourless corpuscles of the human subject.

As the parent cell is forming, the new discs within it gradually become red, and are at length liberated to give origin in like manner to new discs, or to be appropriated in some other way.

6. From § 4 it will be seen that the disc, or so-called "cytoblast," is originally a pellucid globule; which globule therefore is the true cell-germ.

7. Sometimes the quantity of the pellucid substance in the blood-cell is very much increased. This takes place at the expense of the red colouring matter which surrounds it. The blood-corpuscles, now cells, I have seen in various parts collected until the capillaries were completely *filled* with them, and until they had become pressed together into many-sided objects. I have met with vessels at the edge of the crystalline lens, some parts of which presented no other than the pellucid semifluid substance, arisen in the manner now described, and no longer contained within the cells.

8. This originally colourless substance, derived from the nuclei of blood-cells, and nearly filling the capillaries as I have found it, appears to constitute the essential part of coagulable lymph, to organize the same, and to give origin to the tissues, &c. in the manner I have elsewhere described. It seems to be this same originally colourless substance, derived from the nuclei of blood-cells, that forms the exudation-corpuscles of authors, the fibres of false membrane, and the filaments in coagulating blood — filaments which, as I have shown, here and there arise while this substance is still within the cells.

LXIV. *Examination of the Composition of several Mineral Substances. By* Carl Hochstetter*.

Analysis of Augite from Piko, one of the Azores.

THE specimens submitted to examination were found amongst some fragments of decomposed basalt; they consisted of perfectly clean-macled crystals of the usual form. Their specific gravity was = 3·174.

The analysis gave in 100 parts—

		Containing Oxygen.
Silicic acid	50·40	26·17
Protoxide of iron	22·	5·
Lime	21·10	5·92
Magnesia	2·40	0·92
Alumina	2·99	1·41
Loss upon heating	0·30	13·25
	99·19	

* From the *Journal für Praktische Chemie,* No. 22, 1842. Translated and communicated by Mr. E. F. Teschemacher.

The result of the analysis shows that the quantity of oxygen in the silicic acid is twice as great as that contained in the bases, from which the formula $\dot{R}_3 \dddot{S}_2$ results, which perfectly agrees with the composition of augite hitherto examined. The proportion of the bases is however different from any former analysis.

In most of the augites from volcanic districts the quantity of alumina amounts to about 6 per cent., and with it is present a considerable quantity of magnesia, while the protoxide of iron amounts at most to 12 per cent. In these crystals of augite the protoxide of iron and the lime are present in nearly equal atoms, the magnesia and alumina only in small quantities, so that these belong to the class of lime-iron augites*.

Analysis of a new Mineral—Hydrotalcite.

This mineral was examined at the request of Dr. Marchand, who received it from Professor Scheerer; it accompanies the steatite from Snarum, and has the appearance of foliated talc: the first result of the examination showed the entire absence of silex, while talc contains a considerable quantity. It is massive, investing steatite in foliated masses, white, giving a white streak with a mother-of-pearl lustre, transparent, flexible, with a soapy feel; hardness = 2. Heated in a tube it gives off much water; at a red heat it becomes reddish yellow; dissolves nearly completely on boiling with acids.

	Composition.	Containing Oxygen.
Magnesia	36·30	14·15
Alumina	12·	7·27 (Alumina and Peroxide of iron together)
Peroxide of iron	6·90	
Carbonic acid	10·54	7·62
Water	32·66	28·31
Insoluble residue	1·20	
	99·60	

* The analysis by Rose of Hedenbergite from Tunaberg nearly agrees with the above.

Silicic acid	49·01
Protoxide of iron	26·08
Lime	20·87
Magnesia (containing manganese)	2·98
	98·94

D. Red.

The analysis of the reddish-brown malacolite of Dagero in Finland by Berzelius, is still more similar in composition.

Silicic acid	50·
Protoxide of iron	18·85
Protoxide of manganese	3·
Magnesia	4·50
Lime	20·
Loss upon heating	0·90
	97·25

E. F. T.

The analytical result shows that, on account of the insufficient quantity of carbonic acid, the alumina and the oxide of iron must act the parts of an acid and be considered as forming an aluminate with part of the magnesia. This is the view taken of it by Professor G. Rose, who gives the mineral the formula $(3\,\dot{Mg}_2\,\ddot{C}) + (2\,\dot{Mg}_3\,\underline{\dddot{Al}}) + 24\,\dot{H}$. As this mineral is different in composition from any other, it has been named Hydrotalcite, on account of its similarity in its physical characters to talc, from which however it is easily distinguished by the water it contains.

Analysis of Steatite from Snarum.

The composition of the steatite on which the foregoing mineral was found, is—

		Containing Oxygen.
Magnesia	37·52	14·52
Silicic acid	32·03	16·63
Alumina	12·52	7·21 (Alumina and Peroxide of iron together)
Peroxide of iron	4·48	
Water	16·19	14·39
	102·74	

If it is attempted to arrange these different substances, as shown by this analysis, in chemical order, it is evident that, for the expression of a simple formula, there is an excess of silicic acid present. But as the analysis shows an excess of 2·74 per cent., it may possibly arise in the determination of the silicic acid, particularly as the mineral examined was not as pure as could be wished; under these circumstances the following formula would represent the composition—

$$\dddot{Si}_2\,\underline{\dddot{Al}} + 6\,\dot{Mg}\,\underline{\dot{H}},$$

which upon calculation gives—

$2\,\dddot{S}$ =	1154·95	28·71
$\underline{\dddot{A}}$ =	642·33	15·98
$6\,\dot{Mg}$ =	1550·10	38·53
$6\,\underline{\dot{H}}$ =	675·00	16·78
	4022·38	100·

M. R. F. Marchand adds in a note to the above paper, that Dr. Giwartowski from Moscow had analysed the same steatite and found it composed of as follows:—

Magnesia	37·9
Silicic acid	30·2
Alumina	13·2
Peroxide of iron	3·1
Water	17·
	101·4

LXV. *On the Appearances and Relative Positions of the Rocks and Veins which form the Opposite Walls of Cross-Veins.* *By* W. J. Henwood, *C.E., F.R.S., F.G.S., M. Inst. C.E., Member of the Geological Society of France, of the Royal Geological Society of Cornwall, &c.**

[Illustrated by Plate IV.]

IT is frequently asserted as a fact not admitting of dispute, that when one vein traverses another, the severing is newer than the severed vein; and, consequently, that the portions of the latter lying on opposite sides of the former were once united: but, if this be true, it necessarily follows that a perfect and exact coincidence will be found between the dimensions and configuration of these parts; and also that the portions of different veins divided by the same *cross-vein* will be found to preserve, on both sides of it, the same relative distances from each other, either at the same level when the fracture has produced a simple separation, or at different ones when one or both of the severed portions have undergone any motion, either at the time of fracture, or subsequently.

No part of our inquiry is more important than this. For if this coincidence can be established, we have only to ascertain the relative positions of the severed portions of any vein divided by another, and we have a guide to the respective situations of all other veins intersected by the same cross-vein:—at least of all within moderate limits.

The heave of one lode by a cross-vein being known, we have in this coincidence a sure and infallible guide,—an unavoidable, necessary, and unerring law, which will at once indicate in what directions and at what distances all other lodes heaved by the same cross-vein may be re-discovered.

I need not enlarge on the value and importance of such a discovery; for by it the doubts and perplexities with which practical miners have been for ages beset, will be at once removed, and the fruitless cost so often incurred in trying to solve the question of their mutual dependence will for ever be avoided; since the re-discovery of a lode heaved by a cross-vein, instead of requiring experience and observation, will henceforward be merely a matter of simple computation.

But it is not by the examination of any single heave, or of any number of heaves taken each singly, that the truth or falsehood of this position can be established, or that we can make manifest the resemblance or dissimilarity of the divided portions of the same lode, or of the relative distances of different lodes on opposite sides of the same cross-vein. For if

* From the Transactions of the Royal Geological Society of Cornwall, vol. v.

we assume that a motion has taken place to such an extent and in such direction as may be requisite, we may solve, at least within certain moderate limits, the problem of any single heave. The difficulty, however, lies in the mutual connexion of several heaves, for the motion which will reduce one series of displacements to a tolerable continuity may be found to increase our difficulties respecting other heaves by the same cross-vein. It is therefore only by a comparison of several intersections by the same cross-vein, at the same and at different levels, that we can hope to arrive at truth or certainty. In making this comparison I shall confine myself to examples in which the identity of the veins is beyond dispute; and I now proceed to the details which I think will supply ample materials for the confirmation or rejection of this theory.

(*a.*) At Wheal Bolton the middle lode dips towards the south, and the south lode towards the north, yet the cross-course heaves both towards the right-hand,—the former 18 feet, and the latter 12 feet: and beside being so heaved, both of them accompany the cross-course for several fathoms.

(*b.*) At Stray Park the north lode dips towards the south, and the south lode towards the north, yet at certain levels both are heaved towards the right-hand by the Boundary cross-course, and also towards the left-hand by the Machine cross-course; at other levels, however, these lodes are simply intersected by the cross-courses.

At 146 fathoms deep the south lode is 2 feet wide on the western side of the Boundary cross-course, but on its eastern side the lode is 6 feet in breadth: thus whilst the south wall of the lode seems merely intersected, the north one appears to be heaved 4 feet.

(*c.*) At Cook's-kitchen Dunkin's lode dips to the south, and the Middle Engine lode towards the north, but both are heaved to the right-hand by the Little cross-course, which also intersects one elvan-course on the north, and another on the south of the lodes, but heaves neither of them.

(*d.*) At North Roskear the Caunter lode dips southward, and both the Engine and south lodes towards the north, but the cross-course simply intersects the two last, whilst it heaves the first 8 feet towards the left-hand.

(*e.*) At Cardrew Downs the south lode dips to the north, and the north lode towards the south, and between them is an elvan-course dipping south-west, from which several veins strike off into the adjacent slate. The lodes, the elvan-course, and the veins of elvan are all traversed by the Little flucan; but whilst the elvans are simply intersected, both lodes are heaved towards the right-hand,—the former 24 feet, and the

latter 15 feet. In the same mine the Great flucan simply intersects the same elvan-course and its off-shoots, whilst it heaves the south lode 100 feet towards the right-hand.

(*f.*) At Wheal Unity Wood the flucan simply intersects the elvan-course, whilst it heaves Pits-anvollar lode 21 feet, Trefusis lode 12 feet, and the Little ore lode from 30 to 36 feet, all towards the left-hand. The elvan and all three lodes dip northwards.

(*g.*) At Wheal Prudence the elvan-course and the north lode dip to the north, and the south lode towards the south. The cross-course heaves all of them towards the right-hand, the elvan 30 feet, the north lode 9 feet, and the south lode a distance varying from 18 to 30 feet at different levels.

(*h.*) At Polberrow the eastern cross-course heaves the Pye and North Seal Hole lodes each 9 feet, and the South House and Great Gossan lodes each 12 feet: the first two dip towards the north, and the two last towards the south, and all of them at different angles,—nevertheless they are all heaved towards the right-hand.

(*i.*) In the western part of the Consolidated Mines Tiddy's cross-course simply intersects an elvan-course which dips north-west, and also Glover's branch which dips towards the south, with a very variable inclination; but it heaves Michell's lode, which also dips southward, 3 feet towards the right-hand; and Paul's lode, which dips towards the north, from 15 to 18 feet in the same direction. In the corresponding part of the United Mines (south of the Consolidated) the same cross-course heaves the Old lode 8 feet towards the right-hand at one level, and 6 feet to the left at another; it also heaves the Mundic lode, which dips northward, 9 feet towards the right-hand, and Gellard's north and south lodes, which have a southerly dip, each 6 feet towards the left.

(*j.*) The western flucan, in East Wheal Damsel, heaves both lodes 72 feet towards the right-hand, although they dip in opposite directions; whilst Stevens's flucan, at different levels, heaves them to different distances, but always towards the left-hand.

(*k.*) At Wheal Union, the trawn, at 43 fathoms deep, is in three veins, which, above and below that level, are united into one; but, whether separate or united, at their intersections Wheal Gwens lode is always heaved towards the right-hand. Where the trawn is single at one level the heave is 36 ft., at the next 25 ft., and at the third, where it divides, one vein heaves the lode 1·5 foot, the second 8 ft., and the third 24 f. The western trawn heaves the same lode 6 feet in the same direction.

(*l.*) At Wheal Vor, at 40 and 60 fathoms deep, the Main lode is heaved, respectively, 24 and 27 feet by the eastern cross-course, which, at 140 fathoms deep, divides into two veins. The heaves have been already particularized. Those by the western vein, both at 150 and 170 fathoms deep, are 30 feet; and of the heaves by the eastern, the distance is 9 feet at 150 fathoms deep, 5 feet at 170 fathoms, and 7 feet at 180 fathoms. All the heaves, whether by the whole cross-course, or by its separate veins, are towards the left-hand. By Woolf's cross-course, however, the same lode is heaved towards the right-hand, and, on an average, about 50·5 feet. Pl. IV., fig. 8.

(*m.*) At the United Mines, Skues's flucan heaves the Old lode, which dips north 60°—70°, 6 feet; a branch, which inclines south 70°—80°, 6 feet; a second branch, dipping 30°—40° in the same direction, 2 feet; the Middle lode, which is there perpendicular, 5 feet; Michell's lode, which dips south 70°—80°, 6 inches; a branch, which also dips south 70°—84°, 1 foot; and the Mundic lode, which dips north 60°—70°, 7 feet: all these heaves are towards the right-hand. But it heaves in the opposite direction the following lodes:—Bawden's south lode, which dips south 60°—70°, 18 feet; three veins of Bawden's lode, which underlie south 70°—73°, each 18 feet; Nicholls's branch, dipping south 60°—72°, 18 feet; Polkinghorne's lode, which dips north 65°—76°, 6 feet; and lastly, a branch which dips south 60°—74°, 6 feet.

(*n.*) At Herland, Wheal Bailey cross-course, and Chambers's eastern and western flucans heave the lodes towards the left-hand; whilst all the cross-veins west of them, which produce heaves, in the greater number of instances do so towards the right-hand, but few of them to more than an inconsiderable extent.

(*o.*) At Polladras Downs all the lodes dip northward, and the flucan heaves them all towards the right-hand; but the distances are not generally alike for different lodes, or even for the same lode at different levels.

(*p.*) At 18 fathoms deep, in Duffield, the eastern cross-course simply intersects the south lode, whilst it heaves the Little north lode 9 feet towards the left-hand; between these lodes, however, Holman's lode intersects the cross-course, and heaves it 3 feet towards the left-hand. All the lodes have a northerly inclination.

(*q.*) At Fowey Consols, the cross-course simply intersects Bone's and Jeffery's lodes; it next heaves Crosspark lode towards the right-hand, but different distances at different

depths: Williams's lode heaves the cross-course 4 feet towards the left-hand; the cross-course then intersects Trathan's lode, and at some levels heaves it towards the right-hand; and lastly, it heaves the Black lode 11 feet towards the left-hand.

(*r.*) At Tincroft the Old lode dips northward, and simply intersects the eastern cross-course. The same cross-course then heaves Highburrow lode, which dips south, 1 foot towards the right-hand; it next intersects a mass of granite, and also an elvan-course which dips northward, but heaves neither of them. It then crosses two granite veins, one dipping north and the other south, and afterwards Martin's lode, which dips north, heaving all three towards the right-hand. On meeting Dunkin's lode, at 26 fathoms deep, the cross-course is heaved 4 feet towards the right-hand; whilst at 84 fathoms the same cross-course heaves the same lode 4 feet also towards the right-hand; and lastly, the cross-course heaves the south lode, which dips northward 2 feet, in the same direction.

Thus the heaves of two lodes by the cross-course lie between two intersections of the same cross-course by two other lodes; whilst one of the intersections of the cross-course by a lode is included between two heaves of lodes by the cross-course. (Pl. IV. fig. 6.)

(*s.*) In the well-known case at Dolcoath* (Pl. IV. fig. 2), Entral north lode heaves Wheal Bryant cross-course 9 feet towards the right-hand; the same cross-course heaves two veins, one 12 and the other 30 feet, towards the right-hand, and the western portion of the latter is in elvan, whilst the eastern is in slate: the cross-course traverses but does not heave the elvan-course.

In the same mine, at 56 and 76 fathoms deep, the Caunter lode heaves Harriette's lode, respectively, 6 feet and 4 feet; but at 96 fathoms Harriette's lode heaves the Caunter lode 90 feet, and at 116 fathoms, 120 feet. (Pl. IV. fig. 3.)

(*t.*) The Carbona, at Saint Ives Consols, has been already described, and also its intersection by the Middle trawn. It is necessary, however, to repeat here a few particulars. At 80 fathoms deep, the Carbona, deviating from its usual direction (S.E.), takes that of the Middle trawn, and accompanies it for 25 fathoms. Within a few fathoms of this junction the trawn partakes of, and indeed almost assumes, the mineral

* This most important series of intersections was first brought under my notice by the kindness of Capt. Petherick. It is figured in Dr. Boase's Primary Geology, p. 190, fig. 23; and it has also been described by me, Edin. New Phil. Journal, xxii. (1836), p. 163; and Report of the British Association (1837), p. 74.

character of the Carbona. The Carbona then strikes off in a N.E. direction, and leaves the trawn, which soon resumes its original composition. The Carbona*, on the eastern side, is enormously larger than on the western side of the trawn.

(*u.*) At Wheal Providence the eastern flucan heaves the Caunter lode 6 feet towards the right-hand at 30 fathoms deep, and 10 feet towards the left at 48 and 58 fathoms.

(*v.*) At 12 fathoms deep, in Wheal Robert, the cross-course heaves the Ore lode 6 feet towards the left-hand, and at 24 fathoms, 18 feet in the opposite direction.

(*w.*) At West Pink the lead vein heaves Carrow's Gossan lode 5 feet towards the left-hand at 25 fathoms deep; but at 59 fathoms Carrow's Gossan lode heaves the lead vein 60 feet towards the right.

(*x.*) At the Morvah and Zennor Mines the lode and north-course twice intersect each other.

(*y.*) At 112 fathoms deep, in Great Work, the cross-course heaves Wheal Breage lode 1 foot to the left-hand, and at 132 fathoms simply intersects it; whilst at an intermediate spot (122 fathoms) the lode divides the cross-course, although it does not heave it.

(*z.*) At Trevaskus the lode intersects both the eastern and western cross-courses, but it heaves neither of them.

(*aa.*) At Wheal Trannack, the cross-course, at 34 fathoms deep, is perpendicular, and is heaved 2 feet towards the left-hand by the lode; at 64 fathoms it is also perpendicular, and there it is simply intersected; whilst at 74 fathoms it dips east 84°, and is again heaved by the lode 9 feet towards the left-hand.

(*bb.*) At the Marazion Mines, Godolphin, and Dolcoath, some of the cross-veins, which do not appear at the surface, at certain depths below heave the lodes.

(*cc.*) At Relistian, on the other hand, the flucan, from the surface to 125 fathoms deep, intersects, and in one spot heaves, the lode; but beneath that level it wholly disappears.

(*dd.*) At Wheal Buller the cross-veins, which intersect some lodes, although cutting them at all depths, do not reach to other parallel lodes at very small distances from those intersected.

(*ee.*) At Poldory (United Mines) several small flucans in-

* In this most interesting spot some very curious formations of quartz have been lately found. They seem as if they had originally inclosed hexagonal prisms; and these being removed, there remain cups formed of quartz, coated both within and without with small pyramidal crystals of the same substance.

tersect and sometimes heave the lodes; but they do so only in certain parts of their descent, and not only disappear upwards and downwards, but are also of very short lengths, for those which intersect one lode seldom extend to any other: certain cross-veins being, as it were, peculiar to some lodes, and not continuing far from them.

(*ff.*) At Polgooth, Reskilling Great elvan heaves both Saint Martin's and Screed's lodes, whilst Saint Martin's lode intersects the Little elvan, which in every respect but its size resembles Reskilling Great elvan.

(*gg.*) The only other class of facts which seems to bear on this inquiry, but the evidence of which is of an inferential kind, is that comprising the heaves of the lodes at Balnoon and Dowgas, which take place without the intervention of a joint of rock*, or of a cross-vein, or any other intersecting body whatever.

Let us now proceed to consider how far these heaves and intersections can be reconciled with any assumed motions; either on real planes, as those of the cross-veins, or on imaginary ones adopted for the sake of argument alone.

If we grant that the formation of lodes was prior to that of cross-veins,—

I. The intersection of a single lode by a single cross-vein, whether the result be a simple intersection, or a heave to the right-hand or to the left, is easy of solution.

A simple intersection might be effected by the mere opening of a fissure, and the introduction into it of materials forming a cross-vein. Or if the lode were perfectly straight in its descent, the same result would be produced by the elevation of the portion of the lode (and of course of the containing rock also) on one side of the cross-vein, or by the subsidence of that on the other;—provided that the motion took place in a direction parallel to the dip of the lode; as, for example, a vertical motion and a perpendicular lode. But lodes are never perfectly straight, but are curved and irregular when seen in profile.

If, then, there be either an elevation of one side of a cross-vein, or a depression of the other, it would be a circumstance scarcely to be deemed accidental that the curvatures should occur in such regularly recurring and perfectly similar series, and that the degree of motion should be so adapted to them, that one of them should be raised, or the other lowered, so as to preserve a perfect coincidence with another, and thus retain the continuity which, in its primal state, had existed at a different level. Yet these coincidences (if either vertical or

* Dr. Boase, Cornwall Geol. Trans. iv. p. 448.

oblique motion has ever taken place) form 22·7 per cent. of the total number of intersections.

Again, the heave of a lode by a cross-vein may be occasioned either by a horizontal, vertical, diagonal or curvilinear motion of the portion on either side, or on both sides of the cross-vein.

(1.) A simple horizontal motion will occasion an equal displacement at all depths, which almost every fact we have considered (*b*, *i*, *j*, *k*, *l*, &c.) shows to be rarely, if ever, the case.

In all other kinds or directions of motion, the extent or distance of the heave (displacement) depends on the angle included between the line of dip of the lode and that of the direction of the motion, and also on the extent of the motion.

(2.) Let the superficies A B, A^1 B^1, A^2 B^2 (Pl. IV. figs. 1, 2), respectively, be the surface or ground-plan supposed to have formerly been at one level; Y Z, Y^1 Z^1, Y^2 Z^2 the vertical planes exposed by the respective subsidences of A^1 B^1 and A^2 B^2:—a, a^1, a^2 the superficial portions of the same vein which were originally united at the same level; and b, b^1, b^2 the profiles or transverse sections which show the inclinations or downward courses of the respective parts.

Now, supposing the original state restored, and AB, A^1 B^1, A^2 B^2 to be on the same level; let us imagine a fracture to take place in the direction w x, and at the same time the portions A^1 B^1 and A^2 B^2 (which are still united) to subside on the line or in the direction o p. It is obvious that the portion or end of the lode a^1 in the subsided portion A^1 B^1, A^2 B^2 (by subsiding in the direction o p instead of o q, which is the dip of the lode) will not be found at p in contact with o b q, the portion of lode contained in the mass of rock A B, Y Z. Therefore the lode has suffered a heave towards the right-hand, the extent of which is the distance p q; and as the lode is straight, this extent is the same at all levels.

We will now suppose a second fissure to be formed on the line y z, and the portions of rock A B and A^1 B^1, with the lode they contain, to be stationary, and the mass A^2 B^2 to subside still further, but that the line of subsidence is now parallel to the dip of the lode b^1, in the direction of r s, instead of being oblique to it, as in the last instance. As the dip of the lode is perfectly uniform, the superficial portion a^2 will continue in contact with b^1 at s, and consequently there will be no heave at that point.

It needs but little consideration to discover that the very same results must be necessarily obtained whether AB be the fixed mass, and A^1 B^1, A^2 B^2 subside; or whether A^2 B^2 be the stationary portion, and A^1 B^1, A B are elevated: provided only that the lines of motion be still r s and o p respectively.

Neither will it require much inspection to see that it does not signify whether the motion *o p* be vertical or diagonal; for a heave must of necessity take place whenever the lines of motion and of dip do not correspond: and this being the case, the greater the extent of elevation or subsidence, the greater will be the distance of the heave.

On the other hand, it is equally clear that if the lode be perfectly straight, and the lines of motion of the mass and dip of the lode coincide, as in *r s*, the amount of the motion is immaterial; for whatever it may be, no heave can possibly take place so long as this correspondence continues.

Let us now examine a case in which the dip of the lode shall be irregular, and see what should be the direction of the motion which would occasion simple intersections at some levels and heaves at others, but all the heaves in the same direction.

Now it is evident, that with a vertical motion *o p* (Pl. IV. fig. 3), no very obliquely inclined vein can ever be simply intersected at one part of its downward course and heaved at another; for if the direction of the dip remain the same, the direction of the movement tends to separate the severed portions at all parts of their descent. And the lower part of the elevated portion can be brought into contact with, or opposition to, the upper portion of the stationary one so as to cause a simple intersection, only by a change in the direction of the motion, or by reversing the dip of the lode.

If, however, the dip be in opposite directions, in different parts of the lode's descent, then not only may a vertical movement of the portion on one side of the fissure (or cross-vein) produce simple intersections at some depths and heaves at others, but the heaves at different levels may even be in opposite directions; and this is of course the more likely to occur the closer the approach to parallelism between the dip of the lode and the direction of the motion.

Let A B (Pl. IV. fig. 4) denote the original position of the surface, and *a b b* the lode in its original place, some parts of it dipping north, others south. Let A^1 B^1 be the present position of the surface which has been elevated from the level of A B, and a^1 b^1 b^1 the portion of the lode contained in the mass A^1 B^1. Let it also be supposed that A^1 B^1 stands beyond A B, as well as above it; and that they are separated by a cross-vein: and also that the motion has been a vertical one from *p* to *o*. Then at the surface of A B the cross-vein will simply intersect the lode, and will continue to do so as far downward as *b*: from *b* to b^1 the further or elevated portion will stand on the right of the unmoved one, and consequently from *b* to b^1 the heave will be in that direction, although differing in extent at different levels. As portions of the lode

with opposite inclinations are, in consequence of the elevation, opposed to each other on different sides of the cross-vein, at b^1 these severed portions, when viewed in profile, will appear to cross each other, and at that point a simple intersection will again occur. Below b^1, however, the elevated portion will be on the side opposite to that which it occupied above; in fact, there will be a left-hand heave below the point b^1, and a right-hand heave above it.

Though a simple case, this is, however, one of very uncommon occurrence, for I have met with but three examples* of heaves of the same lode by the same cross-vein which are in opposite directions at different levels, viz. at the United Mines, Wheal Robert and Wheal Providence (*i*, *v*, *u*), and as they form so small a proportion, it seems scarcely necessary to pursue this point further.

(3.) Instead of a vertical motion we will now consider one on a line passing through the various flexures of the lode, and which shall, in fact, be coincident with its mean dip.

Let AB (Pl. IV. fig. 5) be the level of the surface originally, $A^1 B^1$ that to which that portion of it has been elevated; a, b, b, b the lode in the former, and a^1, b^1, b^1, b^1 that in the latter, curved as it descends in both; $p\,o$ the direction of the motion. It is obvious, that as a, b, b, b, and a^1, b^1, b^1, b^1 were originally united and continuous, their configuration must be alike; and that, during the motion of $A^1 B^1$ with the lode contained in it, several of the sinuosities of the latter must have passed similar ones in the former, until finally from a to b^1 the heave is towards the right-hand: at b^1 the lodes cross, and there is a simple intersection only; from b^1 to b the heave is towards the left-hand, and at b there is a second simple intersection; from b to b^1 the heave is again to the right, and at b^1 a third simple intersection occurs; from b^1 to b there is again a right-hand heave (for at b^1 the two parts of the veins merely stand opposite to each other, and do not cross); at b there is a fourth simple intersection, and thence downward there is a left-hand heave. This iteration of contradictory phænomena is thus obtained with no greater flexures, in a direction contrary to the general dip, than are common in lodes.

But although these slight reverses often occur, a reversed dip for any great extent is very unusual; and such contradictory heaves are entirely unknown in Cornwall.

(4.) We will now see how far the results of curvilinear motion will agree with observation.

* Mr. Carne describes a case of the kind at Gunnis Lake (Cornwall Geol. Trans. ii. p. 99), and another is said to have occurred at South Wheal Towan.

The centre of motion must be either at the surface or at some point beneath it.

If at the surface, there must necessarily be a simple intersection at that spot (a, Pl. IV. fig. 6); whilst at every inferior station there must be a heave, and the distance of the heave will increase directly as the depth.

The same state of things, but in a reversed order, must also take place should the centre of motion be at some point beneath our reach; for in such a case the heave must be a maximum at the surface, and diminish as we approach the centre.

In either case all the heaves will be in the same direction at all depths.

If, however, the centre of motion be at some point below the surface (but one which has been reached by mining), as at x, Pl. IV. fig. 7, then at that point will there be a simple intersection, and at every other a heave. Here, also, the extent of the heave must of course increase the further it is situated from the centre of motion, whilst all the heaves above that point will be in one direction, and all those below it in the other. Thus, if we suppose the portion a^1, b^1 of the lode to be further from us than a b, then the heave above x will be towards the right-hand, and that below it towards the left.

It has been already stated that we possess but three examples of the heaves of the same lode by the same cross-vein, which are in opposite directions at different levels: two of these (i, v) have been observed at only one spot above, and another below, the neutral point (x): in the third, Wheal Providence, although the direction of the heave is reversed between 30 and 40 fathoms deep, yet at 58 fathoms its extent is the same (10 feet) as at 48 fathoms.

Thus, of the three facts (i, u, v) to which alone this solution can possibly apply, two do not furnish sufficient evidence to determine its application, and the third is inconsistent with it.

(5.) What then must be the conditions of dip and motion, in the heave of a single lode by a single cross-vein, that will occasion simple intersections at some levels and heaves at others, whilst at the same time all the heaves shall be in the same direction?

We have seen that neither a horizontal (1.), vertical (2.), nor curvilinear (4.) motion, nor one coincident with the mean dip of the lode (3.), will satisfy the conditions demanded by the greater number of facts. Let us now assume a motion which, in a single case at least, seems likely to comply with them all; although there seems no reason for presuming on its existence, but that, in the case of individual intersections taken singly, it will comply with many of the facts.

Let AB (Pl. IV. fig. 8) denote the original position of the surface, *a*, *b*, *b* the lode; A^1B^1 the new one to which the piece beyond AB, including the lode a^1, b^1, b^1, has been elevated; and *p o* the line of motion, which includes such an angle with the dip of the lode as to remove every part of a^1, b^1, b^1 so far from the general position of the unmoved portion, that only the more prominent parts of both the segments shall be in contact when they are brought into direct opposition. It is at these salient points only, as at b^1, b^1, that there is a simple intersection, whilst at all other parts of the downward course the heave is towards the left-hand.

We have already seen (3.) that no motion parallel to the dip of the lode will satisfy the conditions before us; though oblique motion, within given limits, will afford an explanation. Yet as the extent of the heave depends on the amount of the motion, if that amount be unlimited, the mean distances of the heaves must progressively increase as they recede from a given point,—a state of things which is not found to prevail.

It is obvious that if the line of motion always includes an angle with the same side of the lode, the heave at all levels must be in the same direction. If, however, the line of motion pass through the undulations of the lode, leaving some of them on one side of it and some on the other, the heave, as previously shown (3.), will also of necessity be sometimes towards one hand and sometimes towards the opposite.

[To be continued.]

LXVI. *On the Occurrence of Trilobites and Agnosti in the lowest Shales of the Palæozoic Series, on the Flanks of the Malvern Hills.* *By* John Phillips, *Esq., F.R.S.*

In the course of the Ordnance Geological Survey of Great Britain the occurrence of organic remains in black shales, very low in the series of the Silurian strata, has been found worthy of much attention by the Director and other members of the Survey, from the information they yield regarding the downward extension of the Salopian fossils into the lowest strata of Wales. On this account the fossils which have been collected from these shales in Pembrokeshire (Abereiddy Bay) and in Caermarthenshire (near Caermarthen, St. Clair's, Mydrim and Llandeilo,) will be found of more importance than from their limited number and generally imperfect preservation could be inferred.

Shales mineralogically undistinguishable from these occur in the Malvern Hills, nearly at the base of the whole Palæozoic series, there exposed under circumstances of much in-

terest, especially in reference to the geological age of the trap rocks with which they are locally associated. Mr. Murchison has described these shales with a careful attention to their position below the great masses of fossiliferous Caradoc sandstones, and (evidently assimilating them in his mind to the black schists of South Wales, whose position appeared nearly similar) sought earnestly for traces of fossils which might justify the collocation of these Malvern shales with the flaggy series of Llandeilo.

In diligent and repeated examinations for the same object, I have been able to add many facts regarding the history of these shales, their place in the Palæozoic series, and their relation to the trappean masses; but it is only within this month that I succeeded in extracting a single trace of fossil organization from their innumerable laminæ. Perhaps but for the accident of a bright sunshine falling on a long crumbling bank of the shales, I might not have resumed what seemed a hopeless search. However, my good fortune prevailed, and I had the pleasure to collect in little weathered bits of the shale abundance of Agnosti and parts of minute Trilobites. I must reserve till a period of more leisure the *description* of these treasures, only remarking that they offer no very obvious analogy with the Llandeilo fossils. The Trilobites are not the Asaphi or Trinuclei of Llandeilo; perhaps the Agnosti are equally peculiar. No Orbiculæ, no Graptolites, no Euomphali (seldom wholly absent from the schists of Caermarthenshire) have been yet found here. Neither does there appear any marked analogy between the fossils of the Malvern shales and those well known in the Caradoc strata incumbent. Upon the whole, I believe it may be recommended to those who are reasoning on the classification of the most ancient fossiliferous strata of Wales, to reserve their final judgement till the Ordnance surveyors have measured the thicknesses of all the distinguishable beds between the Vans of Brecon and the shores of the Menai, and extracted from the slates, shales and conglomerates all the evidence they contain of the systems of life which prevailed during their deposition.

Ledbury, April 11, 1843.

LXVII. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

Nov. 11, 1842. THE following communications were read:—

I. "A few Remarks on the Total Eclipse of the Sun observed at Nice, July 8, 1842." By Captain John Grover.—These will be found in the Monthly Notices of the Society, vol. v. p. 207.

II. "Observation of the Time of the Termination of the Solar Eclipse of July 8, 1842." By Arthur Utting, Esq. Communicated by E. Riddle, Esq.—See Monthly Notices, vol. v. p. 208.

III. "Some Remarks on the Total Eclipse of the Sun on July 8th, 1842." By Francis Baily, Esq., Vice-President of the Society.

It is well known to many members of this Society that I proposed to proceed to the Continent, during the last summer, for the express purpose of observing the total eclipse of the sun which was to take place on the morning of July the 8th, civil reckoning. This object has been accomplished; and I flatter myself that an account of that rare phænomenon, by an eye-witness, may be acceptable to this meeting. A statement of the principal observations that I made was communicated by me to one of the Vice-Presidents of this Society, in a letter written at Milan within 48 hours after the eclipse, whilst the circumstances were still fresh in my memory; and they do not differ from those that I am now about to relate more in detail, and which I am desirous here to place on record.

A total eclipse of the sun, in any particular portion of the globe, is an event of very rare occurrence, since only four or five of these remarkable phænomena are recorded as having been seen in Europe during the last century; to which we may add another that was fortunately seen *at sea*, by Don Ulloa. But the accounts of these several eclipses are by no means satisfactory, since they are discordant in many particulars; which probably has arisen not only from the sudden and unexpected appearances that occurred, but also from the loose description that has been given of them, either by the observers themselves or by those who drew up the accounts, and perhaps did not fully comprehend the intention and meaning of the authors. The difficulty also is very much increased from the want of drawings to represent the exact appearances seen, which are always more readily understood by this method than by any verbal description.

During the present century another eclipse of this kind has taken place in the United States of America, which was observed by Mr. Ferrer; and a minute account of the same, together with a drawing of its appearance, has been published in the sixth volume of the Transactions of the American Philosophical Society. These are the only cases of interest that are on record since the invention of the telescope, within which period we must necessarily limit our attempt to acquire any useful information relative to this remarkable phænomenon. But I must proceed with my narrative.

My original intention was to have taken up my station, for observing the eclipse, at Digne, in the south of France; and I had proceeded on my way thither till I arrived near Lyons, when I found that I had a few days to spare; and as I had proposed to visit Venice before my return home, I altered my route and resolved to proceed in an easterly direction, along the line of the moon's shadow, till the day before the eclipse, when I proposed to halt at the most convenient place that might offer. I therefore turned off towards Chambery, and crossing the Alps at Mount Cenis, passed

through Turin, Asti, and Alessandria, and arrived at Pavia about noon on July 7th.

As this place was directly on the central line of the moon's shadow, I resolved at once to make it my head-quarters. I had intended to apply to the director of the university there, for the use of a convenient place where I might observe the eclipse; but I was agreeably anticipated in this respect by a visit from one of the Professors, who having heard of my arrival and my object, immediately and obligingly came to offer me the use of any one of the apartments in the university that might be considered most adapted for my purpose. On accompanying him to the university with this object, I selected one of the upper rooms of the building, which was admirably adapted for making the observations that I had in view. He then very kindly expressed his readiness to furnish me with any instruments at the university that I might require for my use. But I had taken with me from London the same $3\frac{1}{2}$-feet telescope by Dollond that I had formerly used in the annular eclipse of May 15, 1836, as already described in the tenth volume of the Memoirs of this Society; and I therefore informed him that all I wanted was to be left *alone* during the whole time of the eclipse, being fully persuaded that nothing is so injurious to the making of accurate observations as the intrusion of unnecessary company. Acting upon this hint, he immediately took the key from the outside of the door and placed it in the inside, and told me that I might lock myself in: but there was no occasion for this precaution, for although I heard numerous footsteps pass the door, in their way to an adjoining apartment, which was also used as an observatory on this occasion, no one attempted to enter the room in which I was located.

At four o'clock in the morning of the eventful day I went to the university, in order to prepare for the observation; and at that early hour I found many of the students and official persons walking about. At sunrise a thin stratum of clouds was seen in the east near the horizon, but the sun soon got above this obstruction, and the remainder of the day was beautifully clear and serene; not a cloud was to be seen in any part of the heavens, visible from my window, during the whole time of the eclipse. It was as fine a day as that which I had fortunately witnessed in Scotland, at the annular eclipse of 1836.

I had a very good observation of the commencement and the end of the eclipse; but I did not pay any great attention to these secondary objects, and as my chronometer was not adjusted to correct mean time, these observations can be of no use, except as indicating the duration of the eclipse, which, according to my reckoning, was $1^{h}\ 56^{m}\ 39^{s}\cdot 6$ mean time.

As the moon advanced towards her central conjunction with the sun, I watched very carefully and with much anxiety the approach of the border of the moon towards the still illuminated portion of the sun, which was now rapidly assuming a fine crescent shape, the precursor of total obscuration. I used a *red* coloured glass, in order to observe the phænomenon, notwithstanding the remarks and ad-

vice to the contrary by an American observer; and the power of the eye-glass was about 40. When the total obscuration took place, the coloured glass was removed.

I at first looked out very narrowly for the *black lines* which were seen in the annular eclipse of 1836, as they would probably precede the *string of beads*. These lines however did not make their appearance, or at least they were not seen by me. But the *beads* were distinctly visible; and on their first appearance I had noted down on paper the time of my chronometer, and was in the act of counting the seconds in order to ascertain the time of their duration, when I was astounded by a tremendous burst of applause from the streets below, and at the *same moment* was electrified at the sight of one of the most brilliant and splendid phænomena that can well be imagined; for at that instant the dark body of the moon was *suddenly* surrounded with a *corona*, or kind of bright *glory*, similar in shape and relative magnitude to that which painters draw round the heads of saints, and which by the French is designated an *auréole*.

Pavia contains many thousand inhabitants, the major part of whom were at this early hour walking about the streets and squares, or looking out of windows, in order to witness this long-talked-of phænomenon; and when the total obscuration took place, which was *instantaneous*, there was an universal shout from every observer, which "made the welkin ring," and, for the moment, withdrew my attention from the object with which I was immediately occupied. I had indeed anticipated the appearance of a luminous circle round the moon during the time of total obscurity; but I did not expect, from any of the accounts of preceding eclipses that I had read, to witness so magnificent an exhibition as that which took place. I had imagined (erroneously as it seems) that the *corona*, as to its brilliant or luminous appearance, would not be greater than that faint crepuscular light which sometimes takes place on a summer's evening, and that it would encircle the moon like a *ring*. I was therefore somewhat surprised and astonished at the splendid scene which now so suddenly burst upon my view. It riveted my attention so effectually that I quite lost sight of the string of *beads*, which however were not completely closed when this phænomenon first appeared. I apprehend that only a few seconds of time (perhaps 3 or 4) were wanting to complete the perfect obscuration of the sun; but I cannot speak on this point with much certainty.

I had previously noted down some of the principal objects to which I was desirous of directing my attention during the time of total obscuration, and which seem to have given rise to much discussion on former occasions. These, as far as the *corona* is concerned, had reference principally to its colour, its lustre or paleness, its magnitude and extent, its state of motion or repose, and its encircling the sun or the moon as its centre: then, as to the moon, whether any holes were discernible, or any coruscations of light on the dark side: next, as to the amount of darkness in the atmosphere, the change of colour in surrounding objects, and some other points not requisite here to enumerate further. The time, however, for making accurate obser-

vations of this kind is always so short in total eclipses (in the present case being less than $2\frac{1}{2}$ minutes) that one individual can scarcely attend to all the objects that are requisite to be noticed; more especially if his attention is called away (as in this instance) by any new phænomenon which had not been previously observed, nor even anticipated. It is therefore desirable, in any future occurrences of this nature, that a *division of labour* should be made between two or three observers at the same place; each attending solely to the part which he has selected for his particular object.

The breadth of the *corona*, measured from the circumference of the moon, appeared to me to be nearly equal to half the moon's diameter. It had the appearance of brilliant rays. The light was most dense (indeed, I may say quite dense) close to the border of the moon, and became gradually and uniformly more attenuate as its distance therefrom increased, assuming the form of diverging rays, in a rectilinear line, and at the extremity were more divided and of unequal length: so that in no part of the *corona* could I discover the regular and well-defined shape of a *ring* at its *outer* margin. It appeared to me to have the sun for its centre, but I had no means of taking any accurate measures for determining this point. Its colour was quite white, not pearl-colour, nor yellow, nor red; and the rays had a vivid and *flickering* appearance, somewhat like that which a gas-light illumination might be supposed to assume, if formed into a similar shape. I should think it not impossible to give a tolerable representation of this phænomenon by some artificial contrivance. I have seen something like it, in miniature, by the reflection of the sun's light from a piece of broken glass, and on a larger scale by viewing the sun through a grove of trees; but in both these cases it is necessary to obscure the central portion of the rays. The brilliancy of the *corona* was however quite as great as that which is produced by either of the methods here alluded to. I have annexed hereto a drawing of the *corona*, representing, as nearly as I can preserve in my recollection, the appearance of its shape and extent, and the ramification of the rays at the time of the middle of the total obscuration. I had no time or opportunity for ascertaining the deviation of the moon from the central position of the *corona* at any other point of its progress. (See the copper-plate accompanying this paper, as given in the Monthly Notices.)

Splendid and astonishing however as this remarkable phænomenon really was, and although it could not fail to call forth the admiration and applause of every beholder, yet I must confess that there was at the same time something in its singular and wonderful appearance that was appalling: and I can readily imagine that uncivilized nations may occasionally have become alarmed and terrified at such an object, more especially in times when the true cause of the occurrence may have been but faintly understood, and the phænomenon itself wholly unexpected.

But the most remarkable circumstance attending this phænomenon (at least that which most engaged my observation during the short interval of total obscuration, and drew my attention from

other objects of interest) was the appearance of *three large protuberances* apparently emanating from the circumference of the moon, but evidently forming a portion of the *corona*. They had the appearance of mountains of a prodigious elevation: their colour was red, tinged with lilac or purple: perhaps the colour of the peach-blossom would more nearly represent it. They somewhat resembled the snowy tops of the Alpine mountains, when coloured by the rising or setting sun. They resembled the Alpine mountains also in another respect, inasmuch as their light was perfectly steady, and had none of that flickering or sparkling motion so visible in other parts of the *corona*. All the three projections were of the same roseate cast of colour, and *very distinct* from the brilliant vivid white light that formed the *corona*: but they differed from each other in magnitude. I have endeavoured to represent the appearance of the shape, size, and position of these several protuberances in the accompanying drawing, and have numbered them in the order in which they were first seen by me. My attention was drawn, first of all, to No. 1*, which is situate considerably to the right of the vertical point in the circumference; and on looking round the moon I observed the other two. The largest of them was No. 2, which appeared to be bifurcated, and the separation of the parts was discernible even to the base, so that they might be taken for two distinct projections, one overlaying the other. No. 3 was not quite so large as No. 1. The whole of these three protuberances were visible even to the last moment of total obscuration, at least I never lost sight of them when looking in that direction; and when the first ray of light was admitted from the sun, they vanished with the *corona*, altogether, and daylight was *instantaneously* restored.

I should mention that this drawing represents the appearances as seen in a telescope that *inverts*; and it may be interesting to know that the moon made the first impression on the sun's disc very near the point No. 3, and left it very near the point No. 1. My attention was so constantly taken up by these remarkable and unexpected appearances, that I omitted to watch for the re-appearance of the *beads*, and therefore cannot add my testimony to the re-occurrence of that phænomenon.

The darkness, during the time of total obscuration, was not so great as I had anticipated. I had caused a lighted candle to be prepared, in order to be ready in case of need; but I eventually extinguished it, as I found I could read very small print, and note the time by my chronometer, without its assistance. Prior to the commencement of the eclipse I had observed a great number of swallows flying about; but towards the middle of the eclipse they had all vanished, and did not make their appearance again till a few minutes after the first ray of light emanated from the sun, when they were as active, and soon became as numerous, as ever.

During the time of total obscuration, I examined carefully with

* These numbers refer to a plate illustrating the paper as given in the Monthly Notices.

the telescope the body of the moon, but could not discern any bright spot that might be mistaken for a hole; nor could I discover any coruscations issuing from the dark side of the moon. These, however, were only momentary observations. I was told that several stars were seen, but I could not spare the time to look about for them myself: every moment was occupied with more important matter.

Having thus given a detail of all the principal circumstances that occurred, and precisely in the manner in which they presented themselves to my view, as far as my recollection (committed to paper immediately after the event) will assist me, I had intended to have subjoined to this communication an account of the several phænomena that had been noted on former occasions of this kind, and to have compared the various descriptions with each other, in order to see how far any differences that were observed might be reconciled with present appearances. Or, in other words, to have presented a sort of historical view of the subject, somewhat similar to the plan which I adopted in my memoir relative to the annular eclipse in 1836. But I fear that I may already have encroached too much on the time of the meeting; and I am moreover of opinion that a review of this kind can be taken with greater advantage at a more advanced period of time, when we may be in possession also of the several observations that have been made on the present eclipse at different places on the Continent, and which might thus be introduced into the comparison. Should such a measure be thought desirable and useful to future observers, I may probably intrude again upon the time and attention of the Society.

IV. "Observations of the Total Solar Eclipse of 1842, July 7 (July 8, civil reckoning)." By G. B. Airy, Esq., Astronomer Royal.

In the past summer I made a journey to Turin, principally for the purpose of observing the solar eclipse at a place where it would be total. My intention was rather to observe the nature and succession of the general phænomena of a physical character than to make any precise observations of absolute time or absolute measure, or to attempt to deduce corrections of the elements of the moon's motion. I carried with me a small telescope by Simms, mounted on a short tripod stand, of 1·9 inch clear aperture, and about 14 inches focal length. For the use of this I am indebted to the kindness of Mr. Simms. I had carefully tried it on the sun's disc, and had satisfied myself that it was abundantly competent for the observation of those phænomena of eclipses which have excited so much interest: it is indeed a very good telescope of its size. I had also a duplex pocket-watch.

Having crossed the Alps by the pass of the Little St. Bernard, I had the good fortune, at Cormayeur, to meet Professor Forbes. We made arrangements at once for journeying together to Turin, and for observing the eclipse in concert. We reached Turin late in the evening of July 5.

The next day was spent in the examination of the instruments and localities of the Observatory of Turin, under the auspices of M. Plana, and in the inspection of the hill and church of the Superga.

M. Plana was extremely anxious that we should observe the eclipse at the Observatory, where every facility depending on an ample supply of instruments, and an accurate determination of time, could be afforded us. I had, however, long before fixed on the Superga as a station from which I should desire to see, if possible, the grand phænomena of a total eclipse as exhibited on a large tract of country. It may be proper here to mention that the Superga is the highest point of an insulated cluster of hills, perhaps 800 feet above the Po, and five miles from Turin. It is completely surrounded by the plain of Piedmont, and commands a most remarkable view: on the east over the plain of Lombardy; on the north-east, of Monte Rosa and the neighbouring high Alps; on the north, of the mountains of the Val d'Aosta (Mont Blanc itself is hidden by them); on the west, of Monte Viso and the Dauphiny Alps; and on the south and south-east, of the Maritime Alps and the Apennines; with the plain extending from the foot of the hill to the bases of these mountains in every direction. As the eclipse was to be total for the Superga itself and for the western and southern mountains, and partial for the northern mountains, I had thought it possible that I might see in great perfection the different phænomena in the different parts of the view. The sequel will show that in this respect I was disappointed, but that (by chance) a most important advantage was obtained by my adherence to this plan. Finally, it was arranged that Professor Forbes should make his observations at the Observatory, and that I should go to the Superga.

On the 7th, the observations to be made were fully discussed, and the series of observations intrusted to each person were drawn out in the form of written instructions. The observations bearing upon physical optics were principally consigned to Professor Forbes, those of astronomical character to myself. The unfortunate circumstances of weather, however, rendered the former impossible, and in some degree abridged the latter.

The morning of the 8th, at one or two o'clock (civil reckoning), was very dark and lowering: scarcely a star was visible. I proceeded, however, with a companion to the Superga, and reached it at a short time before five o'clock. Every facility for viewing the eclipse from any part of the church or convent of the Superga that I might select was offered me by the fathers of that establishment; and, had my object been simply to view the country during the eclipse, I should undoubtedly have stationed myself in the upper gallery of the dome of the Superga. But the platform in front of the portico offered far greater facilities for the placing of my telescope, and more of general convenience; and, by moving a few steps, I could at any time command the whole plain. I therefore adopted the platform as my station. I think it due to the courtesy of the Italians to remark, that though many persons were present, I did not receive the smallest interruption of any kind from any one. The sun was clear, and I saw the beginning of the eclipse very well.

The power which I used throughout the eclipse was about 27 (as I have since found by the well-known method of comparing a distant

object seen in the telescope with a near one seen without it). This power was found very convenient. I had intended occasionally to use a higher power, but was prevented from doing so by the following circumstance. The dark glasses adapted to the higher powers were made a little darker than was necessary at Greenwich; not, however, so dark as to prevent the most delicate observation. But, in consequence of the cloudiness of the day of the eclipse, the sun was, for the most part, so faint, that he could scarcely be seen when these higher powers, with their corresponding dark glasses, were employed; and as the glass for the lowest power was necessarily still darker, it was useless to attempt to combine the eye-pieces for the higher powers with the dark glass for the lowest power. I was therefore compelled to lay aside the higher powers. It is certain, however, that the power which I used was sufficient for the nicest observations which the state of the air permitted, as it showed very well the atmospheric undulations on the limbs of the sun and moon; and nothing smaller, of course, could be seen with certainty.

The dark glass which was used on the power actually employed was a combination of a purple and a green glass. It gave to the sun's disc a faint yellow greenish tinge.

As the eclipse advanced (the sun continuing unclouded), I observed a circumstance which I have remarked in every solar eclipse that I have seen. It is, that the limb of the moon is very much more sharply defined than the limb of the sun. This is clearly owing to the difference of intensity of light on different parts of the sun's disc, the intensity near the centre of the disc being much greater than that near the limb, and the degradation very near the limb, though rapid, being gradual. I speak of this as a fact of which I have not the smallest doubt; having long ago observed it in my daily practice as an observer with the transit instrument and the circle; and having also frequently remarked it as an experimenter when I have thrown the image of a portion of the sun's disc upon a small screen, in which case I have always been able to determine whether the limb was approaching to the edge of the fully illuminated screen, simply by the change of the intensity of illumination. And I allude specially to this fact at present, first, because in contemplating the probable phænomena of the eclipse it had been a particular subject of conversation between Professor Forbes and myself; secondly, because it may perhaps assist to explain a very strange observation which I shall shortly have to mention.

I had carried with me a wax-taper in a lantern, which I lighted about the time of the commencement of the eclipse. Whenever I looked round over the country, I also looked at the flame of the taper. I cannot, however, say that I remarked any peculiarity in the colour either of terrestrial objects or of the candle-flame. The flame, as the eclipse advanced, appeared much brighter (and must have been visible to a great distance at the totality), and its colour seemed somewhat redder: but I believe that this change takes place in just the same degree when the general light is diminished from any other cause. The surrounding objects did not receive the greenish

hue which I have remarked in other eclipses of nine or ten digits: I know not whether this was due to the cloudiness of the day, or to the continued correction of my eye by reference to the light of the candle.

I saw no spots whatever on the sun's disc.

About six minutes before the totality I specially recorded the remark that there was a slight undulation on the limbs, but that the cusps were perfectly sharp. I cite this particular observation, because I find it noticed in pencil at the time; but I am quite certain that the cusps were seen perfectly sharp at every other time. The general aspect of objects was now very gloomy.

As the totality approached, the gloom increased very rapidly. About two minutes (or perhaps more) before the totality, my companion exclaimed that it was darker towards the Val d' Aosta. I immediately looked in that direction, and am satisfied that it was not darker there. The country, however, looked blacker on that side, partly, I think, because it is more encumbered with wood, and partly (perhaps) because the mountains are nearer than on the south side, and therefore have less of the whitish atmospheric tinge.

At this time, and to the totality, the appearances were very awful. The gloom increased every moment; the candle seemed to blaze with unnatural brilliancy; a large cloud over our heads, whose appearance I had not particularly remarked, but which, I think, was of cumulo-stratus character, became converted into a black nimbus, blacker, if possible, than pitch, and seemed to be descending rapidly; its aspect became horribly menacing, and I could almost imagine that it appeared animated. Of all the appearances of the eclipse, there is none which has dwelt more powerfully upon my imagination than the sight of that terrible cloud. The sun was very little clouded; his narrow crescent form could be seen with the naked eye when the eyelids were partially closed; there was, however, a dark cloud immediately above him, and fainter clouds about him. Immediately before applying my eye to the telescope to view the completion of the obscuration, I imagined that the light which the sun cast upon the ground was of a reddish colour. But the light was so very faint that I cannot at all vouch for this observation.

I have now to mention a very strange observation. I was viewing the sun most carefully with the dark glass upon the eye-piece, while the small illuminated ring was closing rapidly; my watch was lying on the parapet on which the short telescope-stand was placed, and I was counting its beats, with the intention of observing the time which might elapse between the appearance of Mr. Baily's beads and the total obscurity. I saw the moon's limb advance to the sun's, and cover it completely. I withdrew my eye for a moment from the eye-piece, when I heard my companion remark that the sun was nearly gone. I said firmly, "It is out." On being assured that it was not, I again applied my eye to the telescope, and to my infinite surprise I again saw the narrow ring of the sun's disc, not quite so bright as before. I again saw the moon's limb advance

to the sun's limb, and cover it. In other words, I saw the totality completed twice. With regard to the *fact*, I can only say that I was at the time most fully alive to everything which occurred, and that I was specially prepared for an observation which I expected to be one of the most important in the whole eclipse; and I have not the smallest doubt that the thing occurred, under the circumstances in which it was viewed with my telescope, precisely as I have stated. The *explanation* I cannot offer with great confidence, but I conceive that it may be the following. I have already remarked, that the light of the sun's disc, very near to its limb, is considerably less than in those parts of the disc which are a little further from the limb. This being assumed, it is evident that the interference of a cloud, which was sufficiently dense to hide the faintest part of the disc (at the limb), but not sufficiently dense to hide the brighter parts, would sensibly diminish the sun's diameter. Now I was assured by my companion that there was a cloud upon the sun at the time when I first saw its extinction; and this cloud, though not sufficient to conceal the edge of the sun's disc from the naked eye, might be sufficient to conceal it as viewed in a telescope, in which the specific brightness of any surface is much less than to the naked eye, and which also was armed with a dark glass. But if this explanation is valid, it may apply to many other phænomena. I have frequently seen the sun's limb deeply notched, and I have conceived that this was due to irregularity of refraction; it may have been due to irregularity in the transparency of the atmosphere. Mr. Baily's beads may themselves have depended on this circumstance. I now return to my narrative.

I saw nothing whatever of beads, or other irregularity, in either of the extinctions of the sun's limb. The cusps were perfectly well defined till they met.

I quitted the telescope and looked round the horizon. The outlines of the mountains could with great difficulty be seen. But everything, though not black, appeared horribly gloomy. My companion believed that there was a dark green tinge on every object. I did not remark it. I endeavoured to ascertain whether the darkness could be seen sensibly to travel over the great plain, but could not satisfy myself that it was so; the whole seemed to me to become dark at once. Professors Plana and Forbes, however, on the Turin observatory, (from which the mountains to the north and west are visible, but not the plain,) were confident that they saw the darkness travel gradually. It is possible that from my elevated position I saw the country too much in detail to observe this; it is possible, also, that my eye was applied to the telescope at the critical time. The illumination was so small, that I could with difficulty read the divisions on the watch-plate, which was within eight inches of my eye; I did not try a printed book. The clouds were much less distinct than before, but as far as they could be seen they appeared terribly threatening. But the appearance of the moon can never be forgotten. It was like a black patch fixed in the sky, surrounded by a ring of faint light, whose breadth I estimated at $\frac{1}{8}$th

of the moon's diameter (or probably four minutes). The colour of this ring was nearly white, inclining (as I thought) to peach-colour, but its illuminating power was very small. It was brightest at the lower part and to the left; this brightness travelled a little to the right. (It will be remarked, that a point to the left of the lowest point was the part last covered by the moon.) The clouds, however, were so near to the moon on all sides, and a dense cloud was so nearly in contact with it at the top, that it seems exceedingly probable that some of these appearances might depend upon them.

I gazed earnestly at this remarkable ring, and I could not divest myself of the idea that it was produced by the sun's light shining past the moon's body through a portion of our own atmosphere. I wish it to be understood clearly that I do not offer this as an explanation of the ring (indeed, considering the number of miles by which the moon's limb overpassed the line drawn from the place of observation to the sun's limb, I cannot now consider such an explanation feasible); I wish merely to convey the impression which was given to me at the time of viewing the phænomenon. Indeed, I remarked at the time, that the appearance was almost exactly similar to that produced by a brilliant street-lamp, when its direct rays are just prevented from reaching the eye by a post, or by the corner of a building. I think it possible that there might be a very slight radial appearance in the light of the ring, but I do not recollect it with certainty, and I am perfectly certain that it was not sufficiently marked to interfere sensibly with the general appearance of annular structure. The moon appeared to be extremely near; her distance might have been estimated at a few hundred yards. The whole appearance of things was very unnatural and frightening. No stars were seen from the Superga, the sky being covered with clouds: but I found, from the reports of MM. Plana and Forbes, as well as from the conversation of many persons whom I met in general society, that many stars were seen at Turin, and at other places in the neighbourhood. I may take this opportunity of stating that I heard of distinct instances in which horses exhibited signs of very great terror when the totality came on.

I took off the dark glasses and carefully examined the moon with the telescope. Her disc was distinctly visible as having independent light, and I think that if it had been stronger, I might have seen the large tracts of different brightness on her disc. I could not, however, see the smallest inequality of light, of the nature either of broad dark tract, or dark spot, or bright spot. I looked carefully for a long time (in proportion to the whole duration of darkness), and am confident that there was nothing of this kind to be seen.

While thus looking at the moon I saw, to my great surprise, some small red flames at the apparent bottom of the disc (the top as seen with the naked eye). The number of flames, as I have them impressed on my memory and as I find them drawn on a small pencil sketch made a few minutes after their appearance, was three*;

* A plate representing these and other phænomena of the Eclipse accompanies Mr. Airy's paper, as given in the Monthly Notices.

their form was nearly that of saw-teeth in the position proper for a circular saw turned round in the same direction in which the hands of a watch turn: their height was certainly not greater than one-fourth of the breadth of the ring, or probably a minute: the distance between the first and third was perhaps forty degrees or more on the moon's limb; their colour was a full lake-red, and their brilliancy greater than that of any other part of the ring. On my calling attention to these, my companion saw them with the naked eye. It will be remarked that the part of the limb in which the red flames appeared was immediately in contact with the dark cloud of which I have spoken; and we attributed them to some irregularity in the density of the cloud's edge.

While engaged in watching the reappearance of the sun, I lost the opportunity of observing how the ring and the flames disappeared. Every luminous appearance, however, and every trace of the remainder of the moon's limb, vanished as soon as the smallest portion of the sun was uncovered. No beads or irregularity of any kind could be observed. The general illumination of the earth and sky was restored with very great rapidity. The clouds soon began to cover the sun, and in a short time it was invisible. In less than half an hour, a few drops of rain fell.

My companion, who had better opportunities than I had of observing the formation of the ring, &c., has given me the following account:—

"A bright line seemed to form round the right side of the moon before the disappearance, but not quite round, so that the ring was not complete: but, at the moment of the total disappearance, the ends seemed suddenly to join and form the complete ring, brightest on the left side, and as if beams of light came out. It continued brightest below, and at one time disappeared on the upper side, but a heavy black cloud was touching it there. Shortly before the reappearance, the brightness increased on the upper side; and immediately before the reappearance there were little beams of flame-colour starting out. There was no defined edge to the ring: it changed sensibly, being brightest first on the left side where the sun had gone in, then below, and then on the right side; the light coming out at each place successively like little beams from the moon's edge. There was no remarkable change in the colour of the light till the little flame-coloured beams shot out for a few seconds before the reappearance. The general appearance of the country during the totality was very frightful; but every object, all the distant hills, &c. &c., were distinctly visible; it was like looking at objects through a very dark greenish glass. The sky in every part, except that in which the sun was, was covered with thick clouds. The sun also was covered by the clouds very soon after his reappearance."

The numerous persons who watched the eclipse from the Superga appeared to notice with great interest the progress of its phases to the totality; and when the sun was actually hidden there burst forth from them, first a low murmur and then loud sounds of ap-

plause. Immediately after the restoration of the sun, the whole crowd dispersed, and nobody seemed to regard with the smallest interest the phases of decrease of the eclipse.

The meteorological circumstances of the atmosphere were evidently much affected by the concealment of the sun. The air after the totality appeared transparent (as it usually does when the ground is cold); and there rested upon or near to the flanks of the mountains a series of stratified or cumulo-stratified cloud, at an elevation of less than 2000 feet; with a lower surface very sharply defined and (as far as I could judge) most truly horizontal through the whole extent to which I could see them; and with a less regular upper surface: the depth of these clouds could not be more than 200 or 300 feet. They had not the smallest resemblance to the ill-defined fog-clouds which hang about the mountains at all elevations in rainy weather; nor to any other clouds that I have seen during the day in these countries: I think that I have seen clouds in the evening which resembled them more nearly than any others.

After the termination of the eclipse the day became very hot, and the aspect of the country became very similar to that which it usually presents at this season of the year.

It was not till some hours after my return to Turin that I found that MM. Plana and Forbes had not seen the moon at all during the totality. The region in which the sun and moon ought to have been seen was covered with an opake cloud: I have little doubt that it was the same cloud which I, from the Superga, saw just above the moon (the azimuths of Turin and the moon being almost exactly opposed), and to which I was inclined to attribute some of the peculiar phænomena of the eclipse.

I fear that I have greatly trespassed upon the time of the Royal Astronomical Society; and I can offer only this apology, that in describing a phænomenon of such strange character, of which so few authentic accounts exist, there appears to be no possible way of including all those points which are really of scientific interest, except by narrating everything which was seen, and leaving to others the power of selecting from the mass those circumstances which may possess some real value.

V. Mr. Baily communicated the substance of a circular letter, which he had received from Professor Schumacher, announcing the discovery of a Comet by M. Laugier at Paris, on the 28th of October. At $10^h\ 10^m$ mean time at Paris its right ascension was $16^h\ 41^m$ and its declination $+ 68° \ 44'$. The right ascension increased, in six hours, $3^m\ 34^s$, and the declination diminished $20'$ in the same interval.

VI. Before the close of the meeting, an explanation was given by the Astronomer Royal, Mr. Airy, of the principle of an escapement recently invented by him, and intended to be applied to a clock made by Mr. Dent for the Observatory of Pulkowa. For this we refer to the Monthly Notices, vol. v. p. 221.

ROYAL IRISH ACADEMY.

Nov. 8, 1841.—Sir Wm. R. Hamilton, LL.D., President, in the Chair.

Professor MacCullagh read the following note on some points in the Theory of Light.

I.—*On a Mechanical Theory which has been proposed for the Explanation of the Phænomena of Circular Polarization in Liquids, and of Circular and Elliptic Polarization in Quartz or Rock-crystal; with Remarks on the corresponding Theory of Rectilinear Polarization.*

The theory of elliptic polarization, which I feel myself called upon to notice, was first stated by M. Cauchy, and has been made the subject of elaborate investigation by other writers. That celebrated analyst, conceiving (though without sufficient reason, as will presently appear) that he had fully explained the known laws of the propagation of *rectilinear* vibrations by the hypothesis that the luminiferous æther, in media transmitting such vibrations, consists of separate molecules *symmetrically* arranged with respect to each of three rectangular planes, and acting on each other by forces which are some function of the distance, was led very naturally to imagine that he would find the laws of *circular* and *elliptic* vibrations, in other media, to be included in the more general hypothesis of an *unsymmetrical* arrangement. Accordingly, in a letter read to the French Academy on the 22nd of February, 1836, a letter to which he attached so much importance that he desired it might not only be published in the Proceedings, but also "deposited in the Archives" of that body (see the *Comptes rendus des Séances de l'Académie des Sciences*, tom. ii. p. 182), he gave a precise statement of his more extended views, informing the Academy that he had submitted his new theory to calculation, and that, among other remarkable results, he had obtained (with a slight variation or correction) the laws of circular polarization, discovered by Arago, Biot, and Fresnel. Referring to his Memoir on Dispersion, published at Prague under the title of *Nouveaux Exercices de Mathématiques*, he observes, that the results therein contained may be generalized, by "ceasing to neglect" in the equations of motion [the equations marked (24) in § 2 of that memoir] certain terms which vanish in the case of a symmetrical distribution of the æther. He then goes on to say—

"Nos formules ainsi généralisées représentent les phénomènes de l'absorption de la lumière ou de certains rayons, produite par les verres colorés, la tourmaline, &c., le phénomène de la polarisation circulaire produite par le cristal de roche, l'huile de térébenthine, &c. (*Voir* les expériences de MM. Arago, Biot, Fresnel). Elles servent même à déterminer les conditions et les lois de ces phénomènes; elles montrent que généralement, dans un rayon de lumière polarisée, une molecule d'éther décrit une ellipse. Mais dans certains cas particuliers, cette ellipse se change en une droite, et alors on obtient la polarisation rectiligne." "Enfin le calcul prouve que, dans le cristal de roche, l'huile de térébenthine, &c., la polarisation des rayons transmis parallèlement à l'axe (s'il s'agit du cristal de

roche) n'est pas rigoureusement circulaire, mais qu'alors l'ellipse diffère très peu du cercle."

Thus, to say nothing for the present of the questions of dispersion and absorption, it appears that M. Cauchy conceived he had completely accounted for the facts of circular and elliptic polarization, and that he had deduced the formulas "which serve to determine the conditions and laws of these phænomena." But neither in this letter, nor in any subsequent version* of his theory, has he given the formulas themselves. Nor has he told us the nature of the calculations by which he was enabled to correct the received opinion, and to prove that the vibrations in a ray transmitted along the axis of quartz, or through oil of turpentine, are not rigorously circular as Fresnel and others have supposed, but slightly elliptical. Now—to take the case of quartz—if we consider that the vibrations of a ray passing along the axis are in a plane perpendicular to it, and if we admit, as M. Cauchy always does in the case of other uniaxal crystals, that there is a perfect optical symmetry all round the axis, we shall find it hard to conceive on what grounds he could have come to the conclusion that the vibrations of such a ray are performed in an ellipse. For if all planes passing through the axis of the crystal be alike in their optical properties, there will be absolutely nothing to determine the position and ratio of the axes of the ellipse; there will be no reason why its major axis, for example, should lie in one of these planes rather than in any other. But, whatever may be thought of this case independently of observation, it is manifestly absurd to suppose that the vibrations are elliptical in the case of a ray passing through oil of turpentine, or any other *liquid* possessing the property of rotatory polarization; for, in a liquid, all planes drawn through the ray itself are circumstanced alike. From these simple considerations it is evident that the theory of M. Cauchy is unsound; but a closer examination will show that it is entirely without foundation, and that it is directly opposed to the very phænomena which it professes to explain. To make this appear, however, in the easiest way that the abstruseness of the subject will allow, it will be necessary to advert to some former researches of my own, which have a direct bearing on the question.

The same day on which M. Cauchy's letter was read to the French Academy, I had the honour of reading to the Royal Irish Academy a paper "On the Laws of Double Refraction in Quartz" (see Transactions of the Royal Irish Academy, vol. xvii. p. 461), wherein I showed that everything which we know respecting the action of that crystal upon light is comprised mathematically in the following equations:—

$$\left.\begin{aligned} \frac{d^2\xi}{dt^2} &= \mathrm{A}\frac{d^2\xi}{dz^2} + \mathrm{C}\frac{d^3\eta}{dz^3}, \\ \frac{d^2\eta}{dt^2} &= \mathrm{B}\frac{d^2\eta}{dz^2} - \mathrm{C}\frac{d^3\xi}{dz^3}, \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (1.)$$

* From some statements that have been made within the last few days by Professor Powell (Phil. Mag., S. 3.vol. xix. p. 374), at the request of M. Cauchy himself, it appears that the latter republished his views about cir-

which differ from the common equations of vibratory motion by the two additional terms containing third differential coefficients multiplied by the same constant C, this constant having *opposite* signs in the two equations. The quantities ξ and η are, at any time t, the displacements parallel to the axes of x and y, which are supposed to be the principal directions in the plane of the wave, one of them being therefore perpendicular to the axis of the crystal. The constants A and B are given by the expressions

$$\mathrm{A} = a^2, \quad \mathrm{B} = a^2 - (a^2 - b^2) \sin^2 \psi,$$

where a and b are the principal velocities of propagation, ordinary and extraordinary, and ψ is the angle made by the wave-normal (or the direction of z) with the axis of the crystal. The only new constant introduced is C, which, though the peculiar phænomena of quartz depend entirely on its existence, is almost inconceivably small; its value is determined in the paper just referred to. The equations are there proved to afford a strict geometrical representation of the facts; not only connecting together all the laws discovered by the distinguished observers to whom M. Cauchy refers, and including the subsequent additions for which we are indebted to Mr. Airy, but leading to new results, one of which establishes a relation between two different classes of phænomena, and is verified by the experiments of M. Biot and Mr. Airy. Having, therefore, such conclusive proofs of the truth of these equations, we are entitled to assume them as a standard whereby to judge of any theory; so that any mechanical hypothesis which leads to results inconsistent with them may be at once rejected.

Now I assert that the mechanical hypothesis of M. Cauchy contradicts these equations, and therefore contradicts all the phænomena and experiments which he supposed it to represent. But before we proceed to the proof of this assertion, it may perhaps be proper to remark, that previously to the date of M. Cauchy's communication, and of my own paper, I had actually tried and rejected this identical hypothesis, and had even gone so far as to reject along with it the whole of M. Cauchy's views about the mechanism of light. For though, in my paper, I have said nothing of any mechanical investigations, yet, as a matter of course, before it was read to the Academy, I made every effort to connect my equations in some way with mechanical principles; and it was because I had failed in doing so to my own satisfaction that I chose to publish the equations without comment* as bare geometrical assumptions, and contented myself with stating orally to the Academy, as I did some months after to the

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cular and elliptic polarization in a lithographed memoir of the date of August 1836. But I do not find that he published, either then or since, the detailed calculations which he seems to have made.

* The circumstances here related will account for what Mr. Whewel (History of the Inductive Sciences, vol. ii. p. 449) calls the "obscure and oracular form" in which those equations were published. Having, at that time, no good explanation of them to give, I thought it better to attempt none. But in the general view which I have since taken, they do not offer any peculiar difficulty.

Physical Section of the British Association in Bristol (see Transactions of the Sections, p. 18), that a mechanical account of the phænomena still remained a *desideratum* which no attempts of mine had been able to supply. I am not sure that on the first occasion I stated the precise nature of these attempts, though I incline to think I did; but I have a distinct recollection of having done so on the second occasion, in reply to questions that were asked me by some members of the Association*. Now, my first attempt to explain those equations, which was made almost as soon as I discovered them, actually turned upon the very idea which about the same time found entrance into the mind of M. Cauchy—I mean the idea of an unsymmetrical arrangement of the æther. For as it was generally believed, at that period, that the hypothesis of æthereal molecules symmetrically distributed had led, in the hands of M. Cauchy, to a complete theory of rectilinear polarization in crystals (see his *Exercices de Mathématiques*, Cinquième Année, Paris, 1830, and the *Mémoires de l'Institut*, tom. x. p. 293), the notion of endeavouring to account for the phænomena of elliptic polarization, by freeing the hypothesis from any restriction as to the distribution of the æther, would naturally occur to any one who was thinking on the subject, no less than to M. Cauchy himself. And though, for my own part, I never was satisfied with that theory, which seemed to me to possess no other merit than that of following out in detail the extremely curious, but (as I thought) very imperfect, analogy which had been perceived to exist between the vibrations of the luminiferous medium and those of a common elastic† solid (for it is usual to regard such a solid as a rigid system of attracting or repelling molecules, and M. Cauchy has really done nothing more than transfer to the luminiferous æther both the constitution of the solid and differential formulas of its vibration), still I should have been glad, in the absence of anything

* At the period of this meeting, M. Cauchy's letter on Elliptic Polarization had been published for some months; but I was not then aware of its existence. Indeed the letter appears not to have attracted any general notice: for the theory which it contains was afterwards advanced in England as a new one, and M. Cauchy has been lately obliged to assert his prior claim to it, through the medium of Prof. Powell.—See notes, pp. 400, 405.

† The analogy was suggested by the hypothesis of transversal vibrations, which, when viewed in its physical bearing, was considered by Dr. Young to be "perfectly *appalling* in its consequences," as it was only to *solids* that a "lateral resistance" tending to produce such vibrations had ever been attributed. (Supplement to the *Encyclopædia Britannica*, vol. vi. p. 862. Edinburgh, 1824.) He admits, however, that the question whether fluids may not "transmit impressions by lateral adhesion, remains completely open for discussion, notwithstanding the apparent difficulties attending it." As far as I am aware, Fresnel always regarded the æther as a *fluid*. M. Poisson affirms that it *must* be so regarded, and attributes its apparent peculiarities to the immense rapidity of its vibrations, which does not allow the law of equal pressure to hold good in the state of motion (*Annales de Chimie*, tom. xliv. p. 432). M. Cauchy calls the æther a fluid, though he treats it as a solid. My own impression is, that the æther is a medium of a peculiar kind, differing from all ponderable bodies, whether solid or fluid, in this respect, that it absolutely refuses, in any case, to

better, to find my equations supported by a similar theory, and their form at least countenanced by the like mechanical analogy. Besides, I recollected that Fresnel himself, in his Memoir on Double Refraction, had indicated a "helicoidal arrangement," or something of that sort, as a probable cause of circular polarization (*Mémoires de l'Institut*, tom. vii. p. 73); and as this was an hypothesis of the same kind as the other, only not so general, I was prepared to find that the supposition of an arbitrary arrangement, whatever might be thought of its physical reality, would lead to equations *of the same form* as those which I had assumed. Upon trial, however, the very contrary proved to be the case, for though it was possible to obtain additional terms, containing differential coefficients of the third order, multiplied by the same constant C, yet this constant always came out with the *same* sign in both equations, whereas a *difference* of sign was *essential* for the expression of the phænomena. I had no sooner arrived at this result, than I perceived it to be fatal to the theory of M. Cauchy, and to afford a demonstration of its insufficiency, not only in the particular application which I had made of it, but in all its applications. For the hypothesis which I used was, in fact, identical with that theory, in the most general form of which it is susceptible, when unrestricted by any particular supposition as to the arrangement of the æthereal molecules: and therefore the fundamental conception of the theory could not be true, as it not merely failed to explain a large and most remarkable class of phænomena—those of circular and elliptical polarization—but absolutely excluded them, and left no room for their existence. It followed from this, that the mechanical explanation, which the same theory was supposed to have given, of the phænomena of rectilinear polarization and double refraction in crystals, could not be well founded; indeed, as I have said, I had always distrusted it, and that for various reasons, of which one has been already mentioned, and another was suggested by the forced relations which M. Cauchy had found it necessary to establish among the constants of his theory, and by which he had *compelled*, as it were, his complicated formulas to assume the appearance of an agreement (though, after all, a very imperfect one) with the simple laws of Fresnel.

Such were the conclusions at which I arrived, and the reflections which they forced upon me, nearly six years ago. They have been frequently mentioned in conversation to those who took an interest in such matters, and their general tenor may be gathered from what I have elsewhere written (Transactions of the Academy, vol. xviii. p. 68); but I did not think it worth while to publish them in detail,

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change its density, and therefore propagates to a distance transversal vibrations *only*; while ordinary elastic fluids transmit only normal vibrations, and ordinary solids admit vibrations of both kinds. This hypothesis also includes the supposition that the density of the æther is unchanged by the presence of ponderable matter. As to M. Cauchy's *third ray*, with vibrations nearly normal to the wave, there is no reason to believe that it has even the *faintest* existence; but it is necessarily introduced by his identification of the vibrations of light with those of an indefinitely extended elastic solid.

because it seemed probable that juster notions would prevail in the course of a few years, and that the ingenious speculations to which I have alluded would gradually come to be estimated at their proper value. But from whatever cause it has arisen—whether from the real difficulties of the subject, or the extreme vagueness of the ideas that most persons are content to form of it, or from deference to the authority of a distinguished mathematician—certain it is that the doctrines in question have not only been received without any expression of dissent, but have been eagerly adopted, both in this country and abroad, by a host of followers; and even the extraordinary error, which it is my more immediate object to expose, has been continually gaining ground up to the very moment at which I write, and has at last begun to be ranked among the elementary truths of the undulatory theory of light. Notwithstanding my unwillingness, therefore, to be at all concerned in such discussions, I do not think myself at liberty to remain silent any longer. There are occasions on which every consideration of this kind must give way to a regard for the interests of science.

To show that the principles of M. Cauchy contradict, instead of explaining, the phænomenon of elliptic polarization, let us take the axes of coordinates as before; and let us suppose, for the sake of simplicity, and to avoid his *third ray*, that the normal displacements vanish. Then his fundamental equations take the form

$$\left.\begin{aligned} \frac{d^2\xi}{dt^2} &= \Sigma f \Delta \xi + \Sigma h \Delta \eta, \\ \frac{d^2\eta}{dt^2} &= \Sigma g \Delta \eta + \Sigma h \Delta \xi, \end{aligned}\right\} \quad \cdots\cdots \quad (2.)$$

where f, g, h are quantities depending on the law of force and the mutual distances of the molecules*. If, therefore, we assume that

* I have not thought it necessary to transcribe the original equations of M. Cauchy, which are rather long. He has presented them in different forms; but the system marked (16) at the end of § 1 of his Memoir on Dispersion, already quoted, is the most convenient, and it is the one which I have here used. The directions of the coordinates being arbitrary, I have supposed the axis of z to be perpendicular to the wave-plane. Then, on putting $\zeta = 0$, $\Delta \zeta = 0$, in order to get rid of the normal vibration, the last equation of the system becomes useless, and the other two are reduced to the equations (2.) given above; the letters f, g, h being written in place of certain functions depending on the mutual actions of the molecules. It will be proved, further on, that this simplification does not at all affect the argument. As the directions of x and y still remain arbitrary, I have made them parallel to the axes of the supposed elliptic vibration.

It may be right to observe, for the sake of clearness, that, when the medium is arranged symmetrically, it is always possible to take the directions of x and y such that the two sums depending on the quantity h may disappear from the equations (2.), and then the vibrations are rectilinear. But when the arrangement is unsymmetrical, this is no longer possible.

The equations (2.) are precisely the same as those which have been employed by Mr. Tovey and by Professor Powell, the latter of whom, in his lately published work, entitled "A General and Elementary View of the

each molecule describes an ellipse, the axes of which are parallel to those of x and y; that is to say, if we make

$$\left.\begin{aligned} \xi = p\cos\phi,\ \eta = q\sin\phi,\\ \phi = \frac{2\pi}{\lambda}(st-z), \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (3.)$$

and consequently

$$\Delta\xi = p(\sin 2\theta\sin\phi - 2\sin^2\theta\cos\phi),$$
$$\Delta\eta = -q(\sin 2\theta\cos\phi + 2\sin^2\theta\sin\phi),$$

where $\theta = \frac{\pi\Delta z}{\lambda}$, we shall find, by substituting these values in the equations (2.), which must hold good independently of ϕ,

$$\left.\begin{aligned} s^2 = A' + C'k,\quad s^2 = B' - \frac{C'}{k},\\ \Sigma f\sin 2\theta - 2k\Sigma h\sin^2\theta = 0,\\ \Sigma g\sin 2\theta + \frac{2}{k}\Sigma h\sin^2\theta = 0, \end{aligned}\right\} \quad \ldots\ldots \quad (4.)$$

wherein $k = \frac{q}{p}$ expresses the ratio of the semiaxes of the elliptic vibration, and

$$A' = \frac{\lambda^2}{2\pi^2}\Sigma f\sin^2\theta,\quad B' = \frac{\lambda^2}{2\pi^2}\Sigma g\sin^2\theta,$$
$$C' = \frac{\lambda^2}{4\pi^2}\Sigma h\sin 2\theta.$$

Equating the two values of s^2, we get, for the determination of k, the following quadratic:—

$$k^2 + \frac{A'-B'}{C'}k + 1 = 0. \quad \ldots\ldots\ldots\ldots \quad (5.)$$

Now making the substitutions (3.) in equations (1.), page 400, we have

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Undulatory Theory, as applied to the Dispersion of Light and other Subjects," has dwelt at great length on the theory of elliptic polarization, which they have been supposed to afford, and which he regards as a most important accession to the science of Light. Professor Powell has also made some communications on the subject to the British Association, and has written two papers about it in the Philosophical Transactions (1838, p. 253; and 1840, p. 157), besides several others in the Philosophical Magazine. He, however, always attributed this theory of elliptic polarization to Mr. Tovey, until his attention was directed, by a letter from M. Cauchy, to some investigations of the latter which he had not previously seen (Phil. Mag. S. 3. vol. xix. p. 374). Mr. Tovey set out with the principles of M. Cauchy, and therefore naturally struck into the same track, in pursuit of the same object, apparently quite unconscious that any one had preceded him. It was, indeed, an obvious reflection, that these principles, when generalized to the utmost, ought to include, not only the laws of elliptic polarization, but (as really has been thought by M. Cauchy and his followers) of dispersion and absorption, and, in short, of all the phænomena of optics.

$$s^2 = A - \frac{2\pi}{\lambda} C k, \quad s^2 = B - \frac{2\pi}{\lambda} \frac{C}{k}, \quad . \; . \; . \quad (6.)$$

and thence

$$k^2 - \frac{\lambda}{2\pi C} (A - B) k - 1 = 0, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (7.)$$

a result which is perfectly inconsistent with the former, since the two roots of (5.) have the *same sign*, if they are not imaginary, while those of (7.) have *opposite signs*, and cannot be imaginary. If, therefore, one equation agrees with the phænomena, the other must contradict them. The last equation indicates that, in the double refraction of quartz, the two elliptic vibrations are always *possible*, and performed in *opposite* directions, which is in accordance with the facts; whereas the equation (5.), deduced from M. Cauchy's theory, would inform us that the vibrations of the two rays are either *impossible* or in the *same* direction*.

To apply the results to a particular instance, let us conceive a circularly polarized ray passing along the axis of quartz, or through one of the rotatory liquids, such as oil of turpentine; the position of the coordinates x and y, in the plane of the wave, being now, of course, arbitrary. In each of these cases we have $k = \pm 1$, and $A = B = a^2$, so that the value of s^2 in equation (6.) is expressed by the constant a^2, *plus* or *minus* a term which is inversely proportional to the wave-length λ; the sign of this term depending on the direction of the circular vibration. Now it will not be possible to obtain a similar value of s^2 from the formulas (4.), unless we suppose $A' = B' = a^2$, since it is only in the expansion of C' that a term inversely proportional to λ can be found; but on this supposition the formulas are inconsistent with each other, nor can they be reconciled by any value of k. Indeed, when $A' = B'$, the equation (5.) gives $k = \pm \sqrt{-1}$. Thus it appears that circular vibrations, such as are known to be propagated along the axis of quartz, and through certain fluids, cannot possibly exist on the hypothesis of M. Cauchy. It was probably some partial perception of this fact that caused M. Cauchy to assert that the vibrations in these cases are not exactly circular, but in some degree elliptical; a supposition, which, if it were at all conceivable, which we have seen it is not (p. 400), would be at once set aside by what has just been proved; for no assumed value of k, whether small or great, will in any way help to remove the difficulty.

But this is not all. Rectilinear vibrations are excluded as well as circular; for we cannot suppose $k = 0$ in the equations (4.), so long as the quantity C', resulting from the hypothesis of unsymmetrical arrangement, has any existence. Thus the inconsistency of that hypothesis is complete, and the equations to which it leads are utterly devoid of meaning.

* This conclusion, which shows that M. Cauchy's theory is in direct opposition to the phænomena, might have been obtained without any reference to the equations (1.). But these equations are necessary in what follows.

The foregoing investigation does not differ materially from that which I had recourse to in the beginning of the year 1836. To render the proof more easily intelligible, and to get rid of M. Cauchy's "third ray," which has no existence in the nature of things, I have suppressed the normal vibrations; a procedure which is not, in general, allowable on the principles of M. Cauchy. It will readily appear, however, that this simplification still leaves the demonstration perfectly rigorous in the case of circular vibrations, and does not affect its force when the vibrations are elliptical. For in the rotatory fluids it is obvious that the normal vibrations, supposing such to exist, must, by reason of the symmetry which the fluid constitution requires, be independent of the transversal vibrations, and separable from them, so that the one kind of vibrations may be supposed to vanish when we wish merely to determine the laws of the other. The equations (2.) are, therefore, quite exact in this case; and they are also exact in the case of a ray passing along the axis of quartz, since such a ray is not experimentally distinguishable from one transmitted by a rotatory fluid, and its vibrations must consequently be subject to the same kind of symmetry. In these two cases, therefore, it is *rigorously* proved that the values of k, which ought to be equal to *plus* and *minus* unity, are imaginary, and equal to $\pm\sqrt{-1}$. And if we now take the most general case with regard to quartz, and suppose that the ray, which was at first coincident with the axis of the crystal, becomes gradually inclined to it, the values of k must evidently continue to be imaginary, until such an inclination has been attained that the two roots of equation (5.) become possible and equal, in consequence of the increased magnitude of the coefficient of the second term. Supposing the last term of that equation to remain unchanged, this would take place when the coefficient of k (without regarding its sign) became equal to the number 2, and the values of k each equal to unity, both values being positive or both negative. The vibrations which before were impossible, would, at this inclination, suddenly become possible; they would be *circular*, which is the exclusive property of vibrations transmitted along the axis; and they would have the same direction in both rays, which is not a property of any vibrations that are known to exist. At greater inclinations the vibrations would be elliptical, but they would still have the same direction in the two rays. These results would not be sensibly altered by regarding the equation (5.) as only approximate in the case of rays inclined to the axis; for the last term of that equation, if it does not remain the same, can never differ much from unity; since it must become exactly equal to unity, *whatever* be the direction of the ray, when the crystalline structure is supposed to disappear, and the medium to become a rotatory fluid.

That a theory involving so many inconsistencies should have been advanced by a person of M. Cauchy's reputation, would, perhaps, appear very extraordinary, if we did not recollect that it was unavoidably suggested by the general principles which he had previously adopted, and which were supposed, not merely by himself,

but by the scientific world generally, to have already afforded the only satisfactory explanation of the laws of double refraction in the common and well-known case where the vibrations are rectilinear. This supposed explanation was obtained, as has been said, by restricting the application of M. Cauchy's principles to the hypothesis of a vibrating medium arranged symmetrically, in which case it was shown that the vibrations were necessarily rectilinear; and of course the removal of this restriction was the only way in which it was possible, on those principles, to account for the existence of circular and elliptical vibrations. Accordingly, when M. Cauchy perceived that, on the hypothesis of unsymmetrical arrangement, the existence of rectilinear vibrations became impossible, and that of elliptic vibrations, generally speaking, possible, he found it very easy to persuade himself that he had obtained a new proof of the correctness of his views, and a new and most important application of the fundamental equations by which his general principles were analytically expressed. To have supposed otherwise would have been to admit that his general principles were false. If the elliptical or *quasi*-circular vibrations which he was now contemplating were not capable of being identified with those which had been recognized in the phænomena presented by quartz and the rotatory fluids—if their laws were essentially or very considerably different—his theory would be inconsistent with a wide range of well-known facts, and, notwithstanding its so-called explanations of other laws, should be finally abandoned. Under these circumstances, therefore, he very naturally supposed that his new results *must* be in complete harmony with the phænomena discovered by M. Arago, and analysed so successfully by MM. Biot and Fresnel; although, had he taken the precaution of acquiring such a clear notion of the phænomena as would have enabled him to translate them into analytical language, he must have perceived that they were entirely opposed to his results, and that this opposition furnished an argument which swept away the very foundations of his theory. For, if the constitution of the luminiferous medium were such as M. Cauchy supposes, the well-known phænomena of circular and elliptic polarization would, as we have seen, be absolutely impossible.

Thus the argument which overturns the particular theory of elliptical polarization destroys at the same time all the other optical theories of M. Cauchy, because they are all built on the principles which we have now *demonstrated* to be false. But though the principles of M. Cauchy are now, for the first time, formally refuted, they were objected to, on general grounds, so long ago as the year 1830, by a person whose opinion, on a question of mechanics, ought to have had considerable weight. This was M. Poisson, who, having deduced from the equations of motion of an elastic solid the consequence that such a body admitted vibrations perpendicular to the direction of their propagation, thought it right to remark that this conclusion could not be supposed to account for transversal vibrations in the theory of light, because (as he expressed himself) "the same equations of motion could not possibly apply to two systems

[of molecules] so essentially different from each other" as the æthereal fluid and an elastic solid*. (See *Annales de Chimie*, tom. xliv. p. 432.) The remark, however, did not meet with much attention from mathematicians, who were, perhaps, not disposed to scrutinize too closely any hypothesis which gave transversal vibrations as a result. Besides, the hypothesis appeared to go much further, as it offered *primâ facie* explanations of a great variety of phænomena; it was one to which calculation could be readily applied, and therefore it naturally found favour with the calculator; and as to M. Poisson's objection, it was easily removed by a change of terms, for when the elastic *solid* was called an "elastic *system*," there was no longer anything startling in the announcement that the motions of the æther are those of such a system. The hypothesis was therefore embraced by a great number of writers in every part of Europe, who reproduced, each in his own way, the results of M. Cauchy, though sometimes with considerable modifications. Every day saw some new investigation purely analytical—some new mathematical research uncontrolled by a single physical conception—put forward as a "mechanical theory" of double refraction, of circular polarization, of dispersion, of absorption; until at length the Journals of Science and Transactions of Societies were filled with a great mass of unmeaning formulas. This state of things was partly occasioned by the great number of "disposable" constants entering into the differential equations of M. Cauchy and their integrals; for it was easy to introduce, among the constants, such relations as would lead to any desired conclusion; and this method was frequently adopted by M. Cauchy himself. Thus, in his theory of double (or rather triple) refraction, given in the works already cited (p. 402), he supposes three out of his nine constants to vanish, and assumes, among the other six, three very strange and improbable relations, by means of which each of the principal sections of his wave-surface (considering only two out of its three sheets) is reduced to the circle and ellipse of Fresnel's law; and the three principal sections being thus *forced* to coincide, it would not be very surprising if the two sheets were found to coincide in every part with the wave-surface of Fresnel. The coincidence, however, is only approximate; but M. Cauchy is so far from being embarrassed by this circumstance, that he does not hesitate to regard his own theory as rigorously true, and that of Fresnel as bearing to it, in point of accuracy, the same relation which the elliptical theory of the planets, in the system of the world, bears to that of gravitation (*Mémoires de l'Institut*, tom. x. p. 313). Nor is he at all embarrassed by the supernumerary ray belonging to the third sheet of his wave-surface; he assumes at once that such a ray exists, though it was never seen,

* As the theory of M. Cauchy (*Mém. de l'Institut*, tom. x.) had been communicated to the Academy of Sciences some months before the period (October, 1830) at which M. Poisson wrote, there can be no doubt that M. Poisson's remark was directed against that theory, though he did not expressly mention it.

and promises, for the satisfaction of philosophers, to make known the means of ascertaining its existence (*Ibid.* p. 305). But he afterwards contented himself with observing that as its vibrations are in the direction of propagation they probably make no impression on the eye, and he then gave it the name of the "invisible ray." (*Nouveaux Exercices*, p. 40.)

In these investigations, the suppositions which M. Cauchy had made respecting the constants, led to the result that the vibrations of a polarized ray are *parallel* to its plane of polarization; but in the year 1836 he changed his opinion on this point, and then, by reinstating the constants that he had before supposed to vanish, and establishing proper relations amongst them and the rest, he arrived at the conclusion that the vibrations are *perpendicular* to the plane of polarization (*Comptes Rendus*, tom. ii. p. 342). All his other results, of course, underwent some corresponding change; and it is this new theory which must now be regarded as rigorous, while that of Fresnel is to be looked on as approximate. But it is needless to say, that if the accuracy of Fresnel's law of double refraction is to be disputed, it must be on much better grounds than these; and the results of M. Cauchy are certainly too far removed from that law to have any chance of being consonant with truth. Although, for example, his new views respecting the direction of the vibrations agree, in a general way, with those of Fresnel, there is yet, in one particular, an important difference between them; for according to Fresnel, the vibrations are always exactly in the surface of the wave, while, according to M. Cauchy (in his old theory as well as the new), they are only so in ordinary media. In a biaxal crystal he finds—and this is one of the ways in which the "invisible ray" manifests its influence—that the direction of vibration, in each of the two rays that are visible, is inclined at a certain angle to the wave-plane; but this angle, though small, is by no means inconsiderable, as M. Cauchy seems to intimate, overlooking the fact, which appears from his own equations, that it is of the same order of magnitude as the quantities on which the double refraction depends. It is true, the deviation measured by this angle cannot, if it exists, be directly observed in the refracted light; but its indirect effects on *reflected* light ought to be very great, since the action of the crystal on a ray reflected at its surface differs from that of an ordinary medium by a quantity of the same order merely as the aforesaid angle; and as the problem of crystalline reflexion has been already solved (Transactions of the Royal Irish Academy, vol. xviii. p. 31) on the supposition (which is an essential one in the solution) that the vibrations are *exactly* in the plane of the wave, it is highly improbable, considering the complex nature of the question, that it will be solved, in any satisfactory way, on a supposition so different as that which is required by the theory of M. Cauchy. However, as the laws of such reflexion are now well known, by means of the solution alluded to, it is possible that M. Cauchy may, as in the case of double refraction, succeed in deducing the same laws, or, if not the same, what may seem to be more exact

laws, from certain principles* of his own, helped out, if need be, by proper relations among his constants; especially if, to allow greater scope for such relations, the number of constants be increased by the hypothesis of two coexisting systems of molecules, an hypothesis which M. Cauchy has already considered with his usually generality, but without making any precise application of it. (*Exercices d'Analyse et de Physique Mathématique,* tom. i. p. 33.)

Perhaps one cause why M. Cauchy's views on the subject of double refraction have met with such general acceptance, may be found in the fact, that a theory setting out from the same principles, and leading, by the same relations among constants, to formulas identical in every respect with his earlier results, was advanced independently, and nearly at the same time, by M. Neumann of Königsberg (Poggendorff's *Annals*, vol. xxv. p. 418). A coincidence so remarkable would be looked upon, not unreasonably, as a strong

* In applying these principles to the question of reflexion and refraction at the surface of an *ordinary* medium (*Comptes Rendus*, tom. ii. p. 348), M. Cauchy has arrived at the singular conclusion, that light may be greatly increased by refraction through a prism, at the same time that it is almost totally reflected within it. Supposing the refracting angle of the prism to be very little less than the angle of total reflexion for the substance of which it is composed, a ray incident perpendicularly on one of the faces will emerge making a very small angle with the other face; and as the reflexion at the latter face is nearly total, it is self-evident that the intensity of the emergent light, as compared with that of the incident, must be very small. M. Cauchy, however, finds, by an elaborate analysis, that a prodigious multiplication of light ["*une prodigieuse multiplication de la lumière*"] takes place, the emergent ray being nearly six times more intense than the incident when the prism is made of glass, and nearly nine times when the prism is of diamond. This result was, in a general way, actually verified experimentally by himself and another person; so easy it is, in some cases, to see anything that we expect to see. Had the result been true, it would have been a very brilliant discovery indeed; for then we should have been able, by a simple series of refractions, to convert the feeblest light into one of any intensity we pleased; but the very absurdity of such a supposition should have taught M. Cauchy to distrust both his theory and his experiment. Far from doing so, however, he considers the fact to be perfectly established, and to afford a new argument against the system of emission. "Ici," says he, "un rayon, réfléchi en totalité, est de plus transmis avec accroissement de lumière; ce qui est un nouvel argument contre le système d'émission." The system of emission has at least this advantage, that by no possible error could such a conclusion be deduced from it. For if all the particles of light be reflected, certainly none of them can be refracted.

The truth is, that M. Cauchy mistook the measure of intensity in the hypothesis of undulations, supposing it to be proportional simply to the square of the amplitude of vibration; whereas it is really measured by the *vis viva*, or by that square multiplied by the quantity of æther put in motion, a quantity which in the present case is evanescent, since the corresponding volumes of æther, moved by the ray within in the prism and by the emergent ray, are to each other as the sine of twice the angle of the prism to the sine of twice the very small angle which the emergent ray makes with the second face of the prism. The intensity of the emergent light is therefore very small, as it ought to be, though the amplitude of its vibrations is considerable.

argument in favour of the theory; though it must be allowed that, in the effort to extend the knowledge of any subject, there is a tendency in different minds to adopt the same errors respecting it, as well as the same truths; a fact of which we have seen other examples in the course of the present article.

According to M. Neumann (*Ibid.* p. 454), the "third ray," not being perceived as light, must manifest its existence as radiant heat, or as a chemical power, or as some other agent [" *als strahlende Wärme, oder chemisch wirkend, oder als irgend ein anderes Agens*"], and he thinks that the nature of this ray will be more easily investigated, if the laws of *reflexion* shall be deduced from the aforesaid theory. But we have seen that the laws of reflexion are, to all appearance, at variance with the theory, and they take no account whatever of the third ray. Besides, the discoveries which have been made of late years respecting the polarization of radiant heat, and the strong analogies that have been traced between it and light, amount to a demonstration that its vibrations are transversal, and of course essentially different from those of the supposed third ray, which are normal, or nearly so. There is every reason to believe that the vibrations of the chemical rays are also transversal; and we may confidently assert, that the three species of rays—those of light and heat, and the chemical rays,—are produced not only by vibrations of the same medium, but by the same kind of vibrations, propagated with nearly the same velocities If, therefore, the third ray of MM. Cauchy and Neumann has any existence, it must be referred to "some other agent," the nature of which it is impossible to conjecture.

Enough has now been said to show that the optical theory which we have examined, and which has passed current in the scientific world for a considerable period, is quite inadquate to explain the leading phænomena of light, and that it is based upon principles which are altogether inapplicable to the subject. M. Cauchy states, in the memoir so often quoted (*Mém. de l'Institut,* tom. x. p. 294), that the first application which he had made of his principles was to the theory of *sound,* and that the formulas which he had deduced from them agreed remarkably well with the experiments of Savart and others on the vibrations of elastic solids. As I have already intimated, it is in the solution of such questions (which, however, have long been familiar to mathematicians) that the fundamental equations of M. Cauchy may be most advantageously employed; and had he pursued his researches in this direction, his labours would doubtless have been attended with more success, and with greater benefit to science.

LONDON ELECTRICAL SOCIETY.

[Continued from p. 232.]

Feb. 18, 1843. (Meeting for illustration.)—A Lecture was delivered before the Society, by Mr. Henry Letheby, M.B., A.L.S, &c., "On Animal Electricity," in illustration of the reasons advanced in a paper read at a recent meeting in favour of the theory which

recognizes a closer connexion between nervous energy and the electrical power of certain fishes. This theory is strongly confirmed by the abundant supply of nerves furnished to the electric organ,—by the necessity of integrity in the nerve for producing the electric effect,—by the nervous exhaustion consequent on exercising the power,—by cessation of the animal functions during extraordinary developments of electricity,—and by the muscular effects consequent on passing electricity along the nerves of recently killed animals.

Feb. 21.—Mr. Pollock communicated a series of experiments on electric phosphorescence, which led him to infer that this phænomenon is due to induction, that it is the return of the induced particles to their normal condition, and that conduction, chemical change and colour operate against its production.

Mr. Walker gave a short description of some drawings he had taken of the peculiar appearance presented by a Leyden jar fractured during a powerful discharge of the battery belonging to the Polytechnic Institution, of which this jar formed a part.

Mr. Weekes proposes for the future to employ the condenser or the multiplier in cases of feeble atmospheric development of electricity.

LXVIII. *Intelligence and Miscellaneous Articles.*

ON CERTAIN METALLIC ACIDS.

M. FREMY, in previous memoirs read to the Academy on ferric and stannic acids, thinks he has proved in his researches on the latter, that a metallic acid assumes electro-negative properties only when combined with water, and loses them when rendered anhydrous, and that its capacity of saturation increases with the proportions of water which it contains; he thinks that the opinion is confirmed by the experiments which he has since performed.

Aluminate of Potash.—It is well known that alumina readily dissolves in potash and soda; but no definite compound of alumina and alkali has hitherto been analysed; the analyses of such a compound appears to be important, as it would prove that alumina, in certain cases, acts as an acid; it is also well known that alumina is found in certain minerals in the state of an aluminate. M. Fremy has prepared perfectly crystallized aluminate of potash, and found it to be composed of one equivalent of each of its constituents; it is a hydrated salt, containing two equivalents of water; thus in neutral aluminates the oxygen of the acid is to that of the base as 3 : 1.

Zincate of Potash.—The examination of the compounds of oxide of zinc with the alkalies is attended with great difficulties, they are usually deliquescent and uncrystallizable, but by treating a solution of zincate of potash with a small quantity of alcohol, a salt is obtained in long needles, which M. Fremy considers to be a bizincate of potash. This salt is immediately decomposed by water into anhydrous oxide of zinc and potash which remains in solution.

Protoxide of Tin.—The action of the alkalies on this oxide exhibited some curious peculiarities. According to some chemists a so-

lution of this oxide in an alkali deposits, on evaporation, crystals of metallic tin; according to others it yields anhydrous crystals of protoxide of tin. M. Fremy found that when protoxide of tin is dissolved in a small proportion of alkali, and the liquor is concentrated under the receiver of the air-pump, the alkali at a certain period seizes the water of the hydrate of the protoxide, which then becomes insoluble in the alkali, and is precipitated in the anhydrous state. When, on the contrary, the hydrate of protoxide of tin is dissolved in an excess of alkali, and the solution is rapidly evaporated, the protoxide is converted into stannic acid, which remains combined with the alkali and tin which is precipitated. It is therefore evident that it is the alkali in excess which causes the variation in the decomposition. The foregoing observations show that potash in solution may act upon the water of hydration of an oxide, and render it anhydrous; this experiment induced M. Fremy to examine the influence of other bodies in solution on the hydrate of protoxide of tin, and this examination led to the discovery of the different isomeric states of this protoxide. When hydrated protoxide of tin is boiled with a quantity of potash insufficient to dissolve it, a period arrives at which the precipitate, which has no appearance of crystallization, is suddenly converted into an infinitude of small, brilliant and perfectly black crytals; these are anhydrous protoxide of tin. This oxide differs both in colour and in its crystallization from that prepared by the curious process given by M. Gay-Lussac, which consists in boiling protochloride of tin with excess of ammonia; but these two oxides may be easily reduced to the same state; for if the black oxide prepared with potash be heated in a tube, when the temperature reaches about 390° Fahr. it undergoes a sort of decomposition, the crystals separate violently, increase in volume, change their form, and are converted into the olive oxide, perfectly similar to that prepared by M. Gay-Lussac's process: solutions of some salts possess also the property of dehydrating the protoxide of tin. If the hydrate of protoxide of tin be boiled for a short time in a concentrated solution of chloride of potassium or hydrochlorate of ammonia, the oxide is dehydrated. If a small quantity of hydrated protoxide of tin suspended in a very weak solution of hydrochlorate of ammonia be evaporated, at the moment the salt is precipitated from solution, the hydrate is converted into a powder of a very fine vermilion red; this is, however, protoxide of tin in a new isomeric state. It may be readily converted into the olive-coloured oxide by mechanical agency; for if it be rubbed with the hand, it immediately reassumes the brown colour, which is characteristic of the anhydrous protoxide of tin; so that M. Fremy observes, he has been able to obtain protoxide of tin on three different physical states,—black, olive and red.

Protoxide of tin is not the only oxide which has the property of being dehydrated by the action of the alkalies; when hydrated oxide of bismuth is boiled in an alkaline solution, a period arrives in which the precipitate, which was originally white, is converted into a considerable quantity of small, yellow brilliant needles, which are anhydrous oxide of bismuth.

SPERMATOZOA OBSERVED A SECOND TIME WITHIN THE OVUM.

By Martin Barry, M.D., F.R.SS. L. and E.

Several months since, I communicated to the Royal Society the fact that I had observed, and shown to Professor Owen and others, spermatozoa *within* the mammiferous ovum. The ova were those of the rabbit, taken twenty-four hours *post coïtum* from the Fallopian tube*.

I have this day confirmed the observation; several ova from the Fallopian tube of another of these animals, in a somewhat earlier stage, having presented spermatozoa in their interior: i. e. (as in the first observation) *within* the thick transparent membrane ("*zona pellucida*") brought with the ovum from the ovary.

London, 31 III mo. (March) 1843.

Dr. Forster will publish at Bruges in a few weeks, a treatise on Comets, entitled "Essai sur l'Influence des Comètes sur les Phénomènes de l'Atmosphère terrestre," &c. To which is appended, "Essai sur les Étoiles filantes, avec une catalogue historique."

In March were published at Bruges, "Discours préliminaire à l'Étude de l'Histoire Naturelle," par T. Forster, F.L.S., F.R.A.S., &c. The object of this Treatise is to direct the student of Natural History in the true phrenological method of pursuing the same, and to point out the similarity of principle conspicuous through the organized animal kingdom.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1843.

Chiswick.—March 1. Clear: some snow-flakes: frosty. 2, 3. Clear and frosty: fine. 4. Cloudy and fine: frosty at night. 5. Sharp frost: cloudy. 6. Cloudy: clear and frosty at night. 7. Frosty and foggy: cold with easterly haze. 8. Light clouds: fine: frosty. 9. Dry haze. 10. Hazy: overcast. 11. Slight haze. 12. Uniformly overcast. 13. Clear: cloudy and fine. 14. Fine. 15. Hazy: cloudy and fine. 16. Hazy and mild: clear and fine. 17, 18. Mornings foggy, clear and fine. 19. Foggy: fine. 20. Foggy: very fine: rain. 21, 22. Very fine. 23. Cloudy and mild. 24. Hazy: fine. 25. Dry and windy. 26. Cold and dry. 27, 28. Cloudy and cold. 29. Dry cold haze. 30, 31. Overcast and fine.

Boston.—March 1. Fine: snow early A.M.: rain P.M. 2, 3. Fine. 4. Cloudy. 5. Fine. 6. Cloudy. 7. Fine. 8—10. Cloudy. 11. Fine. 12. Cloudy. 13. Fine. 14. Cloudy: rain early A.M. 15. Fine: rain early A.M. 16. Cloudy. 17, 18. Fine. 19. Cloudy. 20. Fine. 21. Fine: rain early A.M. 22. Rain: rain early A.M. 23, 24. Cloudy: rain early A.M. 25. Windy. 26. Stormy. 27, 28. Windy. 29, 30. Fine. 31. Cloudy.

Sandwick Manse, Orkney.—March 1. Snow-showers: frost. 2—5. Cloudy: thaw. 6. Clear: aurora. 7, 8. Clear: hoar-frost: aurora. 9. Clear: cloudy. 10. Cloudy: damp. 11. Damp. 12. Showers. 13. Snow: showers. 14. Snowing: clear. 15. Snow: showers: clear. 16. Cloudy: snow: rain. 17. Rain: drizzle. 18. Showers: clear: aurora. 19. Cloudy. 20. Cloudy: damp. 21, 22. Damp. 23. Damp: showers: damp. 24. Damp. 25—29. Bright: clear. 30. Cloudy: rain. 31. Drizzle: rain.

Applegarth Manse, Dumfries-shire.—March 1—4. Frost: fair. 5. Slight frost: thaw P.M. 6. Thaw and drizzle. 7. Fair and fine: spring day. 8. Frost. 9. Frost: dull P.M. 10. Rain. 11. Very damp. 12. Wet A.M.: cleared up. 13. Fair and fine: drizzle. 14. Frost: threatening. 15. Frost: fine. 16. Drizzle. 17. Moist, but not rain. 18—20. Fair and fine. 21. Fair and fine: shower P.M. 22. Wet A.M.: cleared. 23, 24. Wet A.M. 25, 26. Fair. 27—29. Fair: slight frost. 30. Heavy rain: thunder. 31. Rain A.M.

* See Proceedings of the Royal Society, Dec. 8, 1842.

Meteorological Observations made at the Apartments of the Royal Society, London, *by the Assistant Secretary*, Mr. Roberton; *by* Mr. Thompson, *at the Garden of the Horticultural Society at* Chiswick, *near London*; *by* Mr. Veall, *at* Boston; *by the* Rev. W. Dunbar, *at Applegarth Manse*, Dumfries-shire; *and by the* Rev. C. Clouston, *at Sandwick Manse*, Orkney.

Days of Month.	Barometer.								Thermometer.										Wind.					Rain.					Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		London: R.S.			Chiswick.		Boston. 8½ a.m.	Dumf.-shire.		Orkney, Sandwick.		London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond. R.S. 9 a.m.
1843. Mar.		Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Fahr. 9 a.m.	Self-reg. Mx.	Self-reg. Mn.	Max.	Min.		Max.	Min.	9¼ a.m.	8¾ p.m.											
● 1.	29·698	29·851	29·676	29·36	29·65	29·86	29·85	30·00	35·8	42·2	34·2	40	23	34	38½	21	32	32	nnw.	ne.	calm	n&ne.	n.	......	......	......	......	·08	34
2.	29·976	30·029	29·961	29·63	29·95	30·00	30·14	30·21	33·7	39·4	31·2	42	24	31·5	38	26	31½	35	nw.	ne.	n.	nne.	nne.	......	......	·20	......	......	31
3.	30·092	30·159	30·070	29·90	30·04	30·16	30·22	30·25	32·3	40·2	31·3	42	30	27	40½	27	37	37½	n.	n.	calm	ene.	nw.	......	......	......	......	......	31
4.	30·336	30·378	30·298	29·94	30·22	30·20	30·23	30·14	35·7	40·0	32·2	41	18	33	42½	20½	37	36	nw.	n.	calm	sw.	sw.	......	......	......	......	......	32
5.	30·416	30·370	30·331	30·02	30·12	30·07	30·00	30·00	31·2	41·8	30·5	46	31	33·5	44	31	43	41	n.	sw.	calm	s.	sw.	......	......	......	......	......	30
6.	30·286	30·269	30·231	29·90	29·99	30·01	30·02	30·09	39·2	43·0	34·0	44	20	36·5	46	37	44	37	s.	s.	calm	s.	wsw.	......	......	......	......	......	32
7.	30·218	30·295	30·182	29·86	30·04	30·09	30·14	30·18	35·7	44·0	32·0	46	24	37	46½	38½	42	38	s.	e.	calm	ese.	s.	......	......	......	0·10	......	30
8.	30·292	30·328	30·264	29·97	30·20	30·24	30·31	30·29	37·7	47·8	33·0	45	28	37	45	25	38½	35½	n.	ne.	calm	sse.	se.	......	......	......	......	......	34
☽ 9.	30·396	30·380	30·318	30·02	30·22	30·12	30·20	30·10	34·8	43·8	33·3	38	24	35	42½	26½	42½	34½	e.	se.	calm	ese.	s.	......	......	......	......	......	31
10.	30·186	30·176	30·050	29·82	29·88	29·90	29·88	29·96	39·3	40·4	34·8	44	28	34	40	34½	43	44	sse.	s.	w.	sse.	s.	......	......	......	......	......	33
11.	30·192	30·153	30·086	29·78	30·00	29·90	30·04	29·78	40·3	44·0	37·6	47	40	38	45½	37	43½	41	s.	sw.	calm	ssw.	n.	......	......	......	......	·08	34
12.	29·952	29·913	29·700	29·53	29·59	29·42	29·56	29·27	43·7	45·6	40·0	50	32	43·5	50½	40½	43½	41	s.	sw.	calm	sw.	sw.	......	......	......	......	·09	39
13.	29·744	29·725	29·585	29·17	29·40	29·28	29·20	29·24	42·7	51·0	39·0	52	43	44	47	37	37½	37½	w.	sw.	w.	wsw.	wnw.	......	·06	......	......	·49	39
14.	29·488	29·605	29·426	28·98	29·30	29·35	29·31	29·55	50·5	51·8	42·7	56	45	47	46	30	35	34	w.	w.	calm	w.		·069	......	·19	......	·20	44
15.	29·790	29·924	29·715	29·33	29·65	29·79	29·75	29·98	46·3	55·5	46·3	57	45	44	48	31	34½	29½	n.	nw.	calm	w.	ne.	......	·06	·07	0·28	·15	44
○ 16.	30·040	29·986	29·940	29·62	29·85	29·70	30·01	29·60	50·5	51·8	46·2	57	29	43	45½	35	33½	37½	sw.	s.	calm	ese.	sse.	·019	......	......	......	......	45
17.	29·920	29·891	29·836	29·39	29·60	29·64	29·46	29·65	46·8	54·2	43·6	64	31	47	46½	41½	45½	43	s.	sw.	w.	sse.	sw.	......	......	......	......	·37	45
18.	29·898	29·861	29·844	29·39	29·73	29·80	29·92	30·04	45·4	59·0	43·4	67	36	44	54	42	40	37	s.	sw.	calm	e-e.	se.	......	......	......	......	·40	45
19.	29·920	29·867	29·771	29·54	29·82	29·80	29·97	29·95	43·3	61·0	41·3	55	41	41·5	56	42	41½	41	ne.	e.	calm	e.	se.	......	......	......	......	......	45
20.	29·588	29·667	29·473	29·33	29·66	29·47	29·84	29·69	48·3	50·8	42·7	63	45	45	49	40	40	40	ne.	e.	calm	ene.	se.	......	·04	......	......	......	45
21.	29·512	29·467	29·379	29·05	29·22	29·24	29·50	29·50	53·8	53·7	48·0	59	47	52	57	40½	41	42	e.	s.	calm	ese.	ese.	......	·21	·01	......	......	49
☾ 22.	29·140	29·409	29·341	28·99	29·15	29·30	29·41	29·43	51·8	59·0	50·4	61	46	50	59	46	44	43	e.	s.	calm	s.	ese.	·163	·02	·01	0·10	·02	52
23.	29·185	29·555	29·416	28·93	29·24	29·48	29·53	29·64	51·3	59·3	51·0	60	42	52·5	56½	47	43	43	sse.	s.	calm	e & s.	se.	......	......	·10	......	......	51
24.	29·604	29·699	29·538	29·19	29·51	29·70	29·93	30·08	53·7	57·6	46·8	64	41	47	51	39½	43½	41	e.	s.	e.	e.	se.	......	......	·01	......	·09	51
25.	29·784	29·734	29·697	29·38	29·90	29·90	30·20	30·32	47·7	60·6	45·0	55	36	45	51	42	42	37	ne.	e.	e.	ene.	se.	......	......	......	......	......	44
26.	29·798	29·756	29·710	29·63	29·98	29·85	30·30	30·32	45·5	53·4	38·7	51	36	44	50	33	42	36	ne.	e.	e.	e.	se.	......	......	......	......	......	37
27.	29·836	29·719	29·700	29·48	29·82	29·81	30·26	30·17	40·0	51·8	37·3	46	38	37	37	34	38½	37	ene.	e.	e.	e.	e.	......	......	......	......	......	35
28.	29·760	29·822	29·722	29·45	29·78	29·84	30·11	30·06	43·5	44·6	39·3	51	28	42	46	33	40	32	ene.	e.	e.	e.	e.	......	......	......	......	......	38
29.	29·966	29·950	29·932	29·63	29·92	29·85	30·07	29·94	42·3	49·0	33·6	56	29	42·5	51½	27	38	32	n.	e.	e.	ese.	e.	......	......	......	......	......	36
● 30.	29·866	29·848	29·602	29·50	29·63	29·20	29·76	29·31	48·8	52·0	38·5	54	42	45	53½	28	40	42	s.	s.	se.	ese.	ne.	......	......	......	0·50	......	39
31.	29·480	29·455	29·384	28·97	29·19	29·20	29·00	29·27	51·8	56·2	48·6	58	46	52	52	44	43	39½	s.	s.	se.	s.	n.	·036	·08	......	......	·49	48
Mean.	29·901	29·912	29·812	29·50	29·750	29·747	29·877	29·883	43·6	50·0	39·6	51·96	31·12	41·4	47·6	34·8	40·09	37·95						Sum. ·287	0·47	0·62	0·98	2·46	Mean. 39

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[THIRD SERIES.]

JUNE 1843.

LXIX. *On some Astringent Substances as Sources of Pyrogallic Acid. By Dr.* John Stenhouse.*

CHEMISTS have usually divided the varieties of tannin which occur so abundantly in the vegetable kingdom, into two kinds; those which give *black* and those which give *green* precipitates with salts of iron. The propriety of this distinction has of late been called in question by Berzelius, who seems to think that the tannic acid in all plants is essentially the same; and that the green and gray colours of the precipitates with salts of iron are owing to the presence of free acid. Professor Liebig appears also to hold a similar opinion. Berzelius states at 116th of his *Rapport Annuel* for 1841, on the authority of C. H. Cavallius, "that the tannic acids which give green precipitates with sulphate of iron, give blue precipitates with acetate of iron, and that their green combinations are rendered blue by the addition either of small quantities of acetate of lead, by a little alkali, or even by a great excess of gelatine." He also states that the lead compounds of those species of tannin which give green precipitates with salts of iron are rendered blue by the addition of a little sulphate of iron. He likewise affirms that when a solution of any of the tannins which give green precipitates remains in contact for some time with chips of iron, a blue instead of a green precipitate is obtained, and that its blue colour is changed to green by the addition of acetic acid. These statements have induced Berzelius to conclude that the giving green or black precipitates with salts of iron is not a distinctive character for any species of tannin, as these precipitates are convertible into each other; bases rendering them black or

* Communicated by the Chemical Society; having been read November 15, 1842.

blue, and acids gray or green. A few experiments which I have made have, however, given somewhat different results from those of Cavallius.

A solution of the tannin of catechu was prepared by macerating a large quantity of catechu in a very little cold water; it was therefore quite free from catechin. A portion of this solution was allowed to remain for some days in contact with a quantity of iron chips; it assumed a dirty, grayish black colour, which, however, did not at all resemble the blue-black colour which nut-galls, oak, bark, &c. exhibit when similarly treated. The precipitate did not become green when a little acetic acid was added to it, but it dissolved in an excess of acid, and on being neutralized with ammonia a purple gray precipitate appeared. Tannin of catechu gave a dull grayish black precipitate with acetate of iron. With neutral acetate of lead it gave a light yellow precipitate, which on the addition of sulphate of iron assumed a dark gray colour, much lighter than the preceding. Basic acetate of lead gave similar results. Chloride of iron gave an olive green; perchloride an olive brown, and protonitrate a yellowish green precipitate. Tannin of catechu, when treated with acetic acid and sulphate of iron, gave a dark olive precipitate.

The tannin of larch bark, which gives a light green precipitate with protosulphate of iron, gave with acetate of iron a purplish black precipitate, which on standing for a day or so assumed a dark lead colour. When treated first with acetate of lead and then with sulphate of iron it gave a grayish purple precipitate. When mixed with a little acetic acid it gave a dark gray precipitate on the addition of sulphate of iron. Chloride of iron produces a grayish brown, and nitrate of iron an olive brown precipitate.

The tannin of gum kino gave with protosulphate of iron a dark green, and with acetate of iron a purplish black precipitate, which on standing changed to grayish black. With protochloride and protonitrate of iron dark green precipitates, which quickly changed to grayish brown. The effect of alkalies on all these precipitates was only to deepen their colours, but not to change any of them bluish black.

The tannin of alder bark, birch bark and tormentil root, gave purplish black precipitates with acetate of iron, which however on standing became grayish black, and their reactions with the inorganic salts of iron were almost identical with those of kino, catechu and larch bark, and wholly dissimilar with those of galls, sumach, valonia, &c. Very good ink of a bluish black colour may be made from nut-galls, oak bark, sumach, valonia, divi-divi, &c.; but the tannin of catechu,

kino, larch, birch and alder barks, &c., are wholly unfit for this purpose. Indeed the only salt of iron which gives pretty nearly the same coloured precipitates with either species of tannin is the acetate, but even these, though at first bluish black in the case of the green tannins, become in a day or two grayish black, and their reactions with the sulphate, chloride and nitrate of iron have no resemblance whatever to those of the black tannins. The old distinction, therefore, which divides the astringents into those which give black, and into those which give green precipitates with the inorganic salts of iron, appears, so far as it goes, to be perfectly just, as these precipitates are not convertible into each other, as affirmed by M. Cavallius. There is, however, good reason for believing that some of the varieties of tannin, which even agree in their reaction upon salts of iron, and in their characters generally, are still by no means identical substances. In this respect there is considerable analogy between the varieties of tannin and the different fatty acids.

It is much to be regretted that we are unable to procure tannin in a state of purity from any other source than nut-galls. When pounded galls are treated by Pelouze's method, with hydrated æther, in a displacement apparatus, the liquid on standing separates into two strata, the lowest of which contains tannic acid in a state of purity.

When, however, oak bark, valonia, sumach, gum kino, catechu, &c. are treated with æther in a similar manner, only one stratum of liquid is obtained. Pelouze's process is therefore inapplicable to these substances. This is the more to be regretted, as from the extreme facility with which tannin decomposes when in contact with moisture, we are unacquainted with any good way of obtaining it in a pure state from any of the other astringent substances; and consequently, with the exception of nut-galls, little progress has been made in the investigation of these bodies.

It has been already mentioned that the tannin of galls and gallic acid are the only substances which, when distilled, are known to yield pyrogallic acid—a substance whose characters are so well marked that it can easily be recognized. It struck me, therefore, that this circumstance might be employed as an easy test for the presence of gallic acid; and also enable us to ascertain when the tannin these substances contained was similar or otherwise with that of nut-galls. With this view I was induced to subject a number of the astringent matters to examination. The first in the order was sumach.

Sumach, which is so extensively employed in Great Britain

by dyers and leather-curriers, consists of the small branches of the *Rhus coriaria*. A quantity of sumach was digested with hot water, filtered and evaporated to dryness. The dried extract thus obtained was subjected to distillation. The liquid which passed into the receiver, though it gave no crystals of pyrogallic acid, obviously contained that substance, as it exhibited all its characteristic reactions. The pyrogallic acid was prevented from crystallizing by the empyreumatic oil and other impurities by which it was accompanied. It seemed not improbable, therefore, that sumach contained gallic acid, and that perhaps its tannin was also similar to that of galls.

The first step taken, therefore, was to examine sumach for gallic. Several pounds of sumach were repeatedly boiled with water and then filtered. The tannin contained in the liquid was precipitated by solution of glue and separated by filtration. Its quantity was very considerable. The clear liquid was evaporated to the consistence of an extract, and treated with hot alcohol. The greater portion of the spirits was recovered by distillation, and the residue set aside to crystallize. After some days, as no crystals made their appearance, the alcoholic solution was evaporated to dryness on the water-bath. It was then introduced into a stoppered bottle and repeatedly agitated with æther; almost the whole of the æther was distilled off, and the residue left to spontaneous evaporation. Abundance of reddish coloured crystals soon appeared. They were purified by repeated digestions with animal charcoal and successive crystallizations. The crystals were then perfectly colourless and possessed the silky lustre of gallic acid, with which acid their reactions with salts of iron and other reagents completely corresponded. When distilled they yielded abundance of pyrogallic acid. They were dried at 212° F. and subjected to analysis.

I. 0·2932 gramme substance gave 0·5315 carbonic acid, and 0·963 water.

II. 0·2824 gave 0·508 carbonic acid, and 0·956 water.

	I.	II.	Calculated.	
C	50·12	49·73	7 C	= 49·89
H	3·64	3·76	3 H	= 3·49
O	46·24	46·51	5 O	= 46·62
	100·00	100·00		100·00

These results approach pretty closely the calculated numbers of hydrated gallic acid given above.

In order to determine the atomic weight of the acid, the basic gallate of lead was formed by adding a solution of the

acid obtained from sumach to an excess of boiling acetate of lead. It precipitated as a yellow, slightly crystalline powder, and was also dried at 212° F.

I. 0·803 salt gave 0·456 oxide and 0·142 metallic lead = 75·84 per cent. oxide of lead.

II. 0·6972 gave 0·332 oxide and 0·186 metallic lead = 76·35 oxide.

Now the bibasic gallate of lead $C^7 H O^3 + 2 Pb O$ contains 76·69 per cent. oxide of lead; therefore there can be no doubt that it was the salt analysed, and that gallic acid, therefore, occurs to a considerable extent ready formed in sumach.

I next proceeded to examine the tannin. In order to obtain the tannin of sumach free from gallic acid, I macerated a very considerable quantity of sumach in a very little cold water, filtered the liquid, and threw down the tannin it contained by adding to the solution about half its bulk of sulphuric acid by small quantities at a time. The precipitate, which had a brownish yellow colour, was tolerably abundant. It was collected on a cloth filter, washed with a little cold water, and strongly compressed so as to remove as much of the sulphuric acid as possible. It was then dried and distilled. It yielded crystals of pyrogallic acid as freely as the same quantity of tannin of galls would have done. The products of the distillation of tannin from both these sources are, therefore, the same.

In order to see if the analogy extended any further, I was induced to try if the tannin of sumach would be converted into gallic acid by being boiled with dilute sulphuric acid, as is the case with tannin when similarly treated. A second portion, therefore, of the tannin of sumach precipitated by sulphuric acid and purified like the former, was boiled for an hour with a mixture of two parts water and one of sulphuric acid. It was filtered while hot. When the liquid cooled, an abundant crop of hard, dark brown crystals appeared. They were collected on a filter and washed with a little cold water, then pressed and dried. They were repeatedly dissolved in small quantities of water, and boiled with purified animal charcoal till they were perfectly colourless. When subjected to analysis,

I. 0·279 acid gave 0·505 carbonic acid, and 0·945 water.

II. 0·3145 gave 0·5675 carbonic acid, and 0·1056 water.

	I.	II.	Calculated.
C	50·04	49·78	7 C = 49·89
H	3·76	3·73	3 H = 3·49
O	46·20	46·49	5 O = 46·62
	100·00	100·00	100·00

These results again agree with the numbers of hydrated gallic acid.

The bibasic gallate of lead was also formed with another portion of the acid in the way already described.

I. 0·690 salt gave 0·153 oxide and 0·3448 lead = 76·18 per cent. oxide.

II. 0·7415 salt gave 0·1745 oxide and 0·3635 lead = 76·34 per cent. oxide.

There can be no doubt, therefore, that the acid produced by the action of sulphuric acid on the tannin of sumach is the gallic acid, precisely as is the case with the tannin of galls. Another portion of the tannin of sumach precipitated also by sulphuric acid, and which had been freed as much as possible, by being washed and compressed, from any adhering acid, was kept well moistened in an open vessel for more than five weeks at a temperature of about 70° F. It readily yielded crystals of gallic acid when treated with alcohol and æther in the way already described. Sumach, therefore, appears to approach the nature of nut-galls more closely than any of the other astringent substances. This fact is well known to Turkey-red dyers, who have long successfully employed sumach as a substitute for galls; as might be expected, a greater weight of sumach is required to produce the same effect, as the quantity of gallic acid and tannin contained in sumach is much less in proportion to its bulk than in nut-galls.

The effect of the action of sulphuric acid upon the tannin, both of sumach and of galls, varies very much according to the strength of the acid employed. When the preparation of gallic acid alone is our object, we succeed best by using acid diluted with seven or eight times its bulk of water. The digestion should be continued for a day or so, new quantities of water being added from time to time as the evaporation proceeds. When the digestion is finished, the liquid should be concentrated by a very gentle heat. Nearly the whole of the tannin is converted into gallic acid, which is deposited in crystals which are only slightly coloured, and therefore easily purified. If, on the other hand, more concentrated acid is used, the crystals are very dark-coloured, and require repeated digestion with animal charcoal, which occasions both trouble and loss. Besides, concentrated acid converts only about a half of the tannin into gallic acid. The other portion is changed into a dark-coloured pulverulent substance possessing decided acid properties, and very much resembling humus in appearance.

Action of Muriatic Acid upon Tannin.

Muriatic acid converts tannin into gallic acid in precisely

the same way as sulphuric acid does. If the muriatic acid is pretty dilute, and the digestion conducted with a moderate heat, the tannin is almost wholly resolved into slightly coloured gallic acid, and a very little only of the insoluble black matter appears. If, on the contrary, we employ an acid diluted with even three times its bulk of water, and continue the boiling for an hour, about half of the tannin is changed into very dark coloured gallic acid, and the rest is converted into the insoluble black matter already mentioned. In order to ascertain that the acid obtained by this process was really gallic acid, it was purified and subjected to analysis. It possessed all the reactions of ordinary gallic acid.

I. 0·2721 gramme substance gave 0·487 carbonic acid, and 0·0865 water.

II. 0·2902 gave 0·5235 carbonic acid, and 0·091 water.

	I.	II.	Calculated.
C	49·49	49·87	7 C = 49·89
H	3·53	3·48	3 H = 3·49
O	46·98	46·65	5 O = 46·62
	100·00	100·00	100·00

In order to determine the atomic weight of the acid its basic lead salt was prepared in the way already mentioned.

I. 0·758 salt gave 0·245 oxide and 0·307 metallic lead = 75·95 per cent. oxide of lead.

II. 0·771 salt gave 0·212 oxide and 0·347 metallic lead = 75·97 oxide per cent.

The calculated quantity of oxide of lead in the bibasic gallate is 76·69 per cent.

From these results there can be no doubt that the acid was gallic acid.

Tannin is precipitated from its solution by muriatic acid even more perfectly than by sulphuric acid, and it is a matter of indifference which of the two acids we employ in order to convert it into gallic acid. The only advantage attending the employment of muriatic acid is that by evaporating the mixture of the two acids nearly to dryness on the water-bath, the greater portion of the muriatic acid may be easily driven off.

Nitric acid does not precipitate tannin from its solution, but almost immediately converts it with evolution of deutoxide of azote into very pure oxalic acid.

The pulverulent substance already mentioned as produced along with gallic acid by the action of muriatic and sulphuric acids upon tannin, has very much the colour of soot and is nearly tasteless. It is insoluble in cold water, and very slightly so in boiling water. When laid on moistened litmus paper,

however, it reddens it strongly. It consists of at least two substances, one of which only is soluble in hot alcohol, so that they may be easily separated by this means. They both dissolve very readily in alkalies, and decompose the carbonates when assisted by heat. When saturated with ammonia and rendered neutral by digestion, they give dark brown precipitates with salts of silver, copper, iron, lead, barytes and lime. Their alkaline solutions are dark brown, and they are completely precipitated by acids. These humus-like substances are produced by the action of the acids on the tannin only, for on boiling gallic acid with concentrated muriatic acid for several hours I did not succeed in obtaining even a trace of them. I am at present engaged in their further investigation.

Valonia.—The next of the astringent matters examined was valonia. It is the acorn of the *Quercus Ægilops*, and is imported in considerable quantities from the Levant for the use of the tanners. The dried extract of valonia prepared like that of sumach, when destructively distilled, gave no indication of pyrogallic acid. Valonia was next examined for gallic acid. A strong solution of it was precipitated by glue, and the clear liquid evaporated to an extract and heated with spirits of wine. The spirits of wine were distilled off, and the residue treated with æther exactly as sumach had been. A very small quantity of crystals was obtained. They exhibited the reactions of gallic acid on salts of iron and other reagents, and when distilled yielded crystals of pyrogallic acid. I have every reason to believe that these crystals were gallic acid, though from the smallness of their quantity I was unable to subject them to analysis. Valonia, therefore, may be regarded as containing a little gallic acid, but its quantity is so inconsiderable as probably not to amount to a thirtieth of what sumach contains.

The most concentrated solutions of valonia give a very scanty precipitate when treated with sulphuric acid; and a large quantity of valonia must therefore be employed to yield any quantity of tannin by this process. The precipitate has a bright yellow colour. When distilled it left a very bulky charcoal, and gave scarcely any empyreumatic products. The liquid which passed into the receiver was nearly colourless, and did not give the least indication of pyrogallic acid. The tannin of valonia appears, therefore, essentially different from that of nut-galls.

Oak Bark.—The extract of oak bark when dried and distilled, also gave no indication of pyrogallic acid. I then endeavoured to obtain gallic acid by treating a decoction of oak bark in the way already described. Though I operated on

considerable quantities, such as six and eight pounds, I did not succeed in the course of several trials in obtaining any crystals of gallic acid. I apprehend, therefore, that if oak bark contains any gallic acid at all, it must exist in very minute quantity. The tannin of oak bark when precipitated by sulphuric acid had a reddish brown colour. When subjected to distillation it gave no indication of pyrogallic acid. The tannin was also boiled with dilute sulphuric acid. It became darker coloured and nearly insoluble either in hot or cold water. It was a little more soluble, though very slightly so, in alkaline leys. On the addition of an acid a few reddish flocks precipitated. Spirits of wine also dissolved a little of it, and assumed a light red colour. The tannin of oak bark appears, therefore, also to differ from that of nut-galls.

Divi-divi.—The astringent substance by some called Divi-divi, by others Libi-divi, has of late years been imported into Great Britain from Carthagena in considerable quantity. It is the pod of a leguminous shrub which grows to the height of between twenty and thirty feet. Professor Balfour informs me that its botanical name is *Cæsalpin coriaria.* It is a native of South America, and is noticed by Dr. McFadyen in his Flora of Jamaica, as occurring in that island. The pods of this shrub, which form the divi-divi of commerce, are of a dark brown colour, nearly three inches long and about half an inch broad. They are very much curled up, as if they had been strongly dried; and contain a few flattish seeds. The taste of divi-divi is highly astringent and bitter. The astringent matter is contained only in the outer rind of the pod; the inner skin, which incloses the seeds, is nearly tasteless. The pods are often perforated with small holes, evidently the work of some insect. The aqueous solution of divi-divi gives a copious precipitate with gelatine, and strikes a deep blue with persalts of iron.

When dried extract of divi-divi was distilled, the liquid which passed into the receiver, though it gave no crystals of pyrogallic acid, evidently contained that substance, as it exhibited all its characteristic reactions.

When divi-divi was treated for gallic acid in the way I have already described, I easily succeeded in obtaining a considerable quantity of reddish-coloured crystals, which, when purified like the others with animal charcoal, became perfectly white. They had the usual reactions of gallic acid, and yielded pyrogallic acid when distilled.

When dried at 212° F. and subjected to analysis,—

I. 0·3034 gramme acid gave 0·550 carbonic acid, and 0·1018 water.

II. 0·3052 gave 0·5505 carbonic acid and 0·1012 water.

	I.	II.	Calculated.
C	50·12	49·87	7 C = 49·89
H	3·72	3·71	3 H = 3·49
O	46·16	46·42	5 O = 46·62
	100·00	100·00	100·00

In order to determine the atomic weight of the acid, I formed the bibasic gallate of lead.

I. 0·706 of this salt gave 0·3325 lead, and 0·1802 oxide = 76·25 per cent. oxide of lead.

II. 0·887 salt gave 0·315 lead and 0·340 oxide of lead = 76·58 per cent oxide.

Now the bibasic gallate of lead $C^7 H O^3 + 2$ P bo contains 76·69 per cent oxide of lead; there can, therefore, be no doubt that it was the salt analysed, and that gallic acid occurs to a considerable extent ready formed in divi-divi.

Sulphuric acid throws down a very scanty dark brown precipitate, even in highly concentrated solutions of divi-divi. When dried and distilled, it did not yield any trace of pyrogallic acid, and left a very bulky charcoal. The tannin of divi-divi appears, therefore, essentially different from that of nut-galls.

Mr. Harvey informs me that a few years ago some calico-printers endeavoured to employ divi-divi as a substitute for galls, but the large quantity of mucilage it contained rendered it unfit for this purpose.

It is at present pretty extensively employed in the tanning of leather, as the quantity of tannin it contains is considerable, and the presence of mucilage is not injurious to that process.

Gum Kino.—The species of kino which I examined was the African variety. I was unable to detect in it any trace of gallic acid. Sulphuric acid threw down the tannin of kino as a bulky dark red precipitate. When distilled, it gave scarcely any volatile products, and no trace of pyrogallic acid. When digested with nitric acid, gum kino was wholly converted into oxalic acid.

Catechu.—It was the light-coloured cubical variety of catechu that I employed. It does not contain any gallic acid, but catechin and a variety of tannin which gives olive green precipitates with salts of iron. Sulphuric acid throws down the tannin of a brownish yellow colour. When boiled with dilute sulphuric acid, it becomes dark brown. Like the tannin of oak bark, it is insoluble either in cold or boiling water. When boiled in strong alkaline solutions, it only dissolves to a very small extent. It is also insoluble in alcohol and æther.

When distilled, the tannin gave no indication either of pyrogallic acid or of pyrocatechin.

Catechin, which is the part of catechu insoluble in cold water, when distilled, yielded the pyrocatechin of Zwenger in considerable quantity. So far as I examined it, pyrocatechin appeared to possess the properties ascribed to it by that chemist.

In conclusion, I may mention that this is only the first of a series of papers on the astringent substances.

Glasgow, Oct. 26, 1842.

LXX. *On some new Cases of Voltaic Action, and on the Construction of a Battery without the use of Oxidizable Metals. By* ALEXANDER R. ARROTT, *Esq.**

HAVING been for some time engaged in examining into certain remarkable voltaic actions occurring in cases hitherto unobserved, or at least not followed out so fully as their importance seems to demand, I am induced briefly to communicate the results of my inquiries, hoping they may not prove uninteresting to the Society.

It is a fact known to every one who has carefully observed the phænomena attending chemical decomposition by means of electricity, that the electrodes immersed in the solution undergoing decomposition, acquire the power of producing a current in the opposite direction to that previously passing through them, when they are made to touch each other without being removed from the liquid.

This effect has generally been ascribed to a power supposed to be acquired by the metals, of producing a current independently of any action of the liquids beyond that of simply completing the circuit, and has been called "polarization of the electrodes."

Becquerel was the first who advanced the opinion, that the effect was due to the alteration produced in the liquid by the current causing decomposition. He supposed that the current was a consequence of the combination of the acid and alkaline produced at the positive and negative surfaces; but it will be found that the most powerful acids and alkalies are incapable of producing a current, unless they readily undergo some other change than that which takes place when an acid and alkali combine as such. It has often been shown, that sulphuric acid and potash, for example, are nearly or altogether

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incapable of producing this effect; iodic, chloric, chromic, or, as in the beautiful arrangement of Becquerel, nitric acid, with an alkali, produce a powerful current, but in these cases the action is very different from that which takes place in simple neutralization of an acid by an alkali.

I have observed, that a current is produced in many cases where, from the nature of the liquids, no current can be supposed to arise either from the union of an acid and an alkali, or from the action of the liquids on the metals employed. Thus I found, that solutions of a per- and protosalt of iron, produced a current when they were allowed to touch each other, and also made to communicate by means of platina; the persalt becoming deoxidized and the protosalt oxidated.

It seemed that in this case the current was due to the oxidation and deoxidation of the liquids, by means of the elements of water which was decomposed, and it appeared probable that, if substances were used capable of exerting a greater attraction for the oxygen and hydrogen, a proportionally greater effect would be obtained. With this view, I tried solution of chlorine, and found that the effect was very much increased. I next tried iodine in solution in water, and also in iodide of potassium: the effect was very feeble; and this is exactly what we ought to expect, for iodine has nearly an equal tendency to unite with oxygen and hydrogen, as appears from the mode in which it decomposes water. I was not aware at the time of making these experiments, that Schoenbein had obtained the current from chlorine.

We have an extremely simple and beautiful illustration of these actions in the case of salts of iron. If two tubes be stopped at one end with plaster of Paris, and filled one with per- and the other with protosulphate of iron, and both immersed in a vessel containing dilute sulphuric acid, on adding red prussiate of potash to the persalt, and sulphocyanide of potassium to the other, no change takes place; but if we connect the two solutions by means of a slip of platina foil, we have instantly indications of the oxidation of the one, and deoxidation of the other. I have also constructed an apparatus in the form of a battery, which, while it serves to illustrate the action in question, may, I think, prove both convenient and economical as an instrument of research in ordinary galvanic experiments. It consists of six small circular jars, within which are fixed tubes of baked clay, or porous earthenware. Small cylinders of platina foil, 0·6th inch diameter, and 1·5 inch long, were placed in the porous tubes, and outside these larger cylinders of 1·8th inch diameter, and 1·5th inch long. The whole was then formed into a series, by connecting the

outer cylinder of the first jar with the inner one of the second, and so on; the porous tube was then filled with strong nitric acid, and the jar with solution of sulphuret of potassium. The arrangement is exactly similar to Daniell's constant battery, except that the metallic surfaces are entirely of platina.

With an instrument of the above dimensions, I have obtained by means of a voltameter, 0·5th cubic inch of the mixed gases in a minute, and that action continues for some hours, with very little diminution.

I find that the substances capable of producing a current in similar circumstances, are very numerous; for example, per- and protosalts of iron, tin, and manganese, an alkaline sulphuret hyposulphite, hypophosphite or a hydracid on the one side, and chlorine or chromic or nitric acid on the other.

The intensity of the effect is, however, very various in these different combinations; thus, with salts of iron it is very feeble; while with chlorine or nitric acid and an alkaline sulphuret, the intensity is such, that the induction of one pair of plates is sufficient to cause the decomposition of water.

Each of the combinations, it will be observed, is formed of an oxidating and a deoxidating substance, and the change which takes place is similar in all of them; the oxidating substance is reduced and the deoxidating is oxidated.

If we employ only one substance, for example—chlorine, the chlorous element of the water finding nothing with which it can unite, is evolved; but in that case the intensity of the action is very much reduced.

The mode in which the experiments were performed was very simple. A small vessel of baked clay was cemented inside a wine glass; the respective liquids were then poured into this vessel and into the glass, till they stood at the same level; in this way they were in free liquid contact, while their actual mixture proceeded with extreme slowness; metallic plates, which were in all cases of platina, were then plunged into the solutions; the plates had been carefully cleaned with nitric acid, then with potash, and washed with water.

I now proceed to state the conclusions at which I have arrived, as regards the law which regulates the action in the ordinary voltaic battery, and in the arrangements above mentioned.

This I find to be in strict agreement with that of ordinary mechanical forces, viz. that the action and reaction are equal and opposite. When a metal is reduced from its solution, the equal reaction seems to follow as a corollary to the law of definite electrolyzation, and in cases where no solid substance is deposited, the same law holds.

In order to prove this, and also to show that the effect is not dependent on any particular state of the metals, a porous vessel was filled with a *mixture* of strong solutions of proto- and per-sulphates of iron; this vessel was then placed in another filled with the same mixture, a platina plate was plunged into each vessel, and the circuit completed by a delicate galvanometer; not the slightest effect was produced; the plates were then put in communication with the poles of a common battery, and the amount of current which passed measured by means of a voltameter. No gas was evolved in the solutions, nor was any iron reduced, but the persulphate increased in quantity at one side, and the protosulphate at the other. When 80 measures of gas were collected, the battery was removed, and the plates being connected by the galvanometer as at the beginning of the experiment, a powerful current was produced in a direction the reverse of that of the battery; the effect was the same whether the same or fresh plates were used, and if they were simply washed with water, they might be moved from one vessel to the other, without producing the slightest effect on the current, provided their connexion with the galvanometer was also changed. The arrangement was again connected with the battery, in such a manner that the current should move in the solutions in the opposite direction to the former battery current, 80 measures of gas were again collected, and the battery being removed, not the slightest current was observed on connecting the plates with the galvanometer, but every thing was in the same state as at the commencement of the experiment. The power of the solution to produce the current gradually diminishes, but is not entirely destroyed until the second or return current becomes equal in amount to the first. The quantity of the *return* current cannot be measured correctly, without using the battery to enable it to pass quickly; because, from the extreme slowness of the action towards the end of the experiment, the solutions unavoidably mix, and thus cause great error in the amount. A similar experiment was tried with nitric acid, containing a large quantity of the lower oxides of nitrogen with precisely similar results.

Where the circuit is entirely metallic, but not homogeneous, we have also a return current; for the heat produced in a thermoelectric arrangement causes a current the reverse of that which produces it; but we cannot in this case determine the amount, from the impossibility of retaining the heat produced, and of preventing it from extending to those parts which ought to remain cold; the same may be the case also in a homogeneous circuit, but from the extremely low inten-

sity, which is only equal to that necessary to cause the current to pass through it, we are unable to observe it.

From these and other observations of a similar kind, I believe it may be stated as a general law, that when a current passes through a series of conductors, it induces a state of things, capable of producing a current equal to itself in amount, and opposite in direction; provided the changes produced are permanent.

This law can be proved to hold in all cases where a liquid forms part of the circuit, with the single exception of the case where two pieces of the same metal communicate by means of one of its salts; here the phænomena are the same as if the metallic circuit were complete. (Faraday.)

These results appear to show that there is something of the nature of a force transmitted through the circuit, and the phænomena of tension lend great support to this idea, for here we have bodies actually put in motion.

If now we suppose each chemical molecule to be capable of exerting an attractive force on every other molecule in its vicinity, the phænomena of the voltaic circuit are precisely what should result from such an attraction, and voltaic action appears to be chemical action under another form; the action in one case taking place between molecules in contact, or at extremely small distances, and in the other between those at a considerable and sensible distance.

If any number of molecules of different substances be placed near each other, and in such a state as to admit of their free motion, they arrange themselves so that their attractive forces produce a state of equilibrium, and till this state is attained the molecules are in a constrained condition. Thus when chlorine, hydrogen and water are brought into contact, the hydrogen and chlorine unite to form hydrochloric acid, and this is the state of equilibrium. The mode in which this state is attained seems to be as follows:—(Fig. 1.) 

; the atom of Cl unites with the H previously in combination with the O as water, while the O unites with the free H, and Cl H and H O are formed; if, however, the H and Cl are at some distance from each other, as when separated by water, the (Fig. 2.) 

action cannot take place, for the molecules cannot assume such an arrangement as would form a complete circuit, without which they cannot exert their attractions, but if H and Cl be united by a metal (Fig. 3.)

a body of an atomic constitution similar to the liquid, the circuit is completed, H Cl is formed, and equilibrium restored. The attraction which was before exerted between H and Cl was unobserved on account of their extreme nearness, but is now continued through the whole length of the circuit, and we have thus the means of observing the phænomena to which it gives rise.

If C l be the only free element, the molecules arrange themselves thus:—(Fig. 4.) 

, the Cl and H unite, and the O is set free; if in place of Cl and O being in contact at the moment of the evolution of O, they are at some distance, the intervening space being occupied by a metal, we have the same phænomena as in the first case, except, that the O finding nothing with which it can unite, is set free at the surface of the metal; the action in this case proceeds much more rapidly than when no metal is used, in consequence of the great attraction between the zincous and chlorous atoms of the platina, for the attraction throughout the circuit is equal to the most powerful exerted at any part of it. It is not necessary that the atoms acting in this manner should be elementary substances, for compound substances, such as cyanogen, many neutral salts and organic substances, act in the same manner.

The result of the action is the same, whether we simply mix the liquids, or form them into a voltaic circuit as above described. If, for example, we mix one equivalent of Cl and Sn Cl, one equivalent of $Sn\ Cl_2$ is formed. If, instead of mixing the solutions, we place them in porous vessels, immersed in hydrochloric acid, and connect them by a slip of platina, the result is the same as before, and the quantity of acid remains the same, having served merely as the means of connecting the solutions, and might be dispensed with, were it not, that the unavoidable mixing which takes place when the solutions are in contact, renders the result very unsatisfactory. If, in place of the above-mentioned solutions, we substitute the per- and protochloride of iron, the formation of per- and protochloride continues till the quantity of these salts is equal in the two vessels.

It thus appears that the result is the same as would be produced by diffusion.

The action as before mentioned is similar with H and Cl; with Cl and HO, HS, HI, KO, KS, KI, we have a chloride

of hydrogen, or of the metal formed, and the combined radical is evolved. This law is a general one, and holds good equally in ordinary chemical action; but here the result is modified from the circumstances in which the action takes place. Thus when Cl is added to KO, no oxygen is evolved; but this disappearance of the oxygen is entirely a secondary result; the oxygen at the moment of its evolution is in contact with Cl and KO, by which it is absorbed, giving rise to the chlorate of potash: but no such result can occur in the voltaic arrangement; for the oxygen, at the moment of its evolution, is not in contact with Cl, and therefore appears as gas. Another difference between voltaic and chemical action is, that in the former, substances unite which are without action on each other when simply mixed, for example—oxygen and hydrogen; but this arises from the powerful attraction of the molecules of the platina, re-acting on the hydrogen and oxygen. The intensity is thus raised to the point necessary to cause combination.

It will now be evident why no current should result from the union of an acid and an alkali; thus if hydrochloric acid and potash be in contact, we have simply an exchange of the elements (Fig. 5.) 

Cl unites with K, while the liberated O unites with the H of the hydrochloric acid, and the current is confined within the four elements, which cannot in this case be separated; the same is true of sulphuric acid, substituting SO_4 for Cl. Nitric, chromic and several other acids are capable of acting in a very different manner from this. The whole atom of acid $NO_6 + H$ is capable of acting in the same manner as Cl, uniting with the basyle of the substance in contact with it, and evolving the radical, as in the pile of nitric acid and potash of Becquerel, but in this case the acid is reduced to the state of peroxide of nitrogen or nitric oxide. The increase in the intensity of the action, by using potash in contact with nitric acid, seems to be due to the affinity of the potash, for an additional quantity of oxygen and the formation of peroxide of potassium, which is immediately decomposed by the water; on substituting caustic barytes, no oxygen is evolved.

The changes which take place when organic substances are arranged so that a current is formed, afford an interesting subject for inquiry, and it appears probable that many of the effects described as catalytic, are the result of actions of this nature; for it is not necessary that the substance connect-

ing the liquids should be a metal; any conducting substance will answer the same purpose; nor is it necessary that the substance should have a sensible size, for a single atom may produce a new arrangement of the molecules of the substance with which it is in contact, as in the case of platina with oxygen and hydrogen, when these substances are mixed, by the intensity of the attraction of its atoms, and this arrangement will depend on that of the substance causing decomposition; the arrangement of the atoms composing the ordinary molecules being similar in both.

It would thus appear that voltaic action is nothing more than chemical action taking place in circumstances that enable us to observe many of the phænomena to which it gives rise, and which we have no means of observing in ordinary cases; and that chemical action is the result of the tendency of the molecules to arrange themselves in a state of equilibrium, in the same manner as ordinary mechanical forces.

LXXI. *On the Combination of prolonged direct luminous Impressions on the Retina with their complementary Impressions. By Sir* David Brewster, *K.H., D.C.L., F.R.S., and V.P.R.S. Edin.**

IT is well known that when we have looked steadily at an object for some time, and then shut our eyes, the object continues visible, and of its natural colour, or the impression of it is prolonged on the retina *during* the *third* part of a second; and it is equally well known that if the object is coloured and pretty luminous, it will appear in its *accidental* or *complementary* colours *after* the *third of a second* has elapsed. This last phænomenon is easily seen, but I have met with many persons who have never seen distinctly the first phænomenon, unless in experiments such as those exhibited by the *thaumatrope*, and similar pieces of apparatus.

In making some of these experiments in the morning before the eye has had its sensibility diminished by exposure to light, I have observed a singular *combination* of these two phænomena, which I believe has not been previously noticed.

If, when our eyes have been for a few minutes shut, we open them and look steadily at the pattern of a carpet, suppose a *red* pattern upon a *green* ground, and then suddenly shut our eyes, we shall see the *red* pattern upon a *pinkish-green* ground, the *red* being very deep and approximating to *black*.

* Communicated by the Author.

The picture is beautifully distinct, but of very short duration, and is not succeeded by any *accidental* or *complementary* impression, owing to the carpet being faintly illuminated. The *pink* ground is obviously a combination of the original *green* ground with its very faint complementary *red*, while the *dark red* pattern has had its *redness* deepened by its own complementary *green*.

When this experiment is well made, which can be done only when the eye is very sensible to luminous impressions, the observer feels as if he possessed *two eyelids* which are closed in succession, with an interval of *one third of a second*, the *first* or the *real* eyelid shutting out the *original* object, and the *second* or the *imaginary* eyelid shutting out the picture composed of the *prolonged direct impression*, and its coexisting *complementary impression*. If the eyes are kept very steady with the intersection of their axes fixed upon the centre, or any other definite point, of the *red* pattern, the eyelids may be shut and opened any number of times in succession without injuring the brightness and distinctness of the combined impression. The picture indeed becomes more and more distinct, and if a considerable degree of illumination is employed, the *single* complementary impression may be made visible after the component one has vanished, or rather after the *prolonged direct impression* has disappeared from the *compound* one.

St. Leonard's College, St. Andrew's,
April 29, 1843.

LXXII. *Remarks on* Mr. Joule's *Explanation of Experiments on the Galvanometer. By Sir* Graves C. Haughton, *F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I HAD the pleasure of reading Mr. Joule's explanation of my experiments on the galvanometer which are printed in your Journal for March; and while I perfectly agree with him that the movements observed in the needles were due to repulsion, I think that the means I took to ascertain that fact, and which were known to me previous to reading his letter, may be more convincing than the experiment which he gives in illustration, though they are really founded on the same principle.

Observing that the needles were instantly attracted by the

finger or any neutral body that was brought near them, while they stood at an angle of 90°, and even to give a smart, stinging shock, it was obvious that the insulation of the galvanometer had prevented the escape of the electricity, and though it was passing off at the other end of the wire that remained coiled up, yet still so considerable an accumulation had taken place, that every part of the galvanometer, including the needle, was highly charged; and that consequently by the law of *similar states* the needle placed itself at right angles to the frame of the galvanometer, being equally repelled by both its ends, and consequently took up that position, in which, to use mathematical language, there was an equilibrium of forces. This view suggested to me that the cause of the very small magnetic needle (mentioned in my former letter) not being moved, was owing to its shortness, as much as to its polarity, and accordingly I mounted a fine straw, one inch and a quarter in length on each of its points, and placed it in the galvanometer, when it was affected more sensibly than any other needle whatever, as might have been expected from its delicate construction. To put the matter however beyond all doubt, a sewing-needle was run through a slip of cork, upon which the needles were suspended in succession, and placed upon a plate of glass. One end of a piece of copper wire one foot and a half long and one eighth of an inch in diameter, was then placed on the cork and in *contact* with the sewing-needle, while the other end rested against the prime conductor, and immediately upon the machine being set in motion, any needle employed placed itself instantly at right angles to the copper wire. Whatever inclination was given to the wire, the needle under trial placed itself at right angles to it, which in fact was the only position it could assume, owing to the charged state of the wire and the needle.

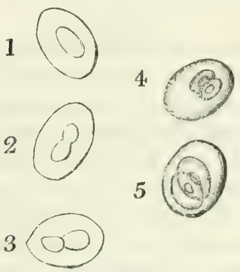
These experiments, as well as those of M. Becquerel, which I have already alluded to, show that the effects of heat and of repulsion ought carefully to be guarded against in the use of the galvanometer; and that whenever there is any degree of insulation, some repulsion may be anticipated, and which being added to the magnetic influence of the wire will give greater amplitude to the deflection of the needle, than would otherwise be the case.

I am, Gentlemen, your obedient Servant,

April 8, 1843. GRAVES C. HAUGHTON.

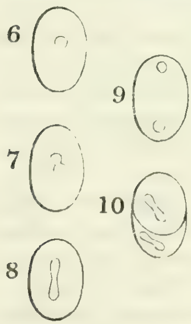
LXXIII. *The Cells in the Ovum compared with Corpuscles of the Blood.—On the difference in Size of the Blood-corpuscles in different Animals.* By Martin Barry, *M.D., F.R.SS. L. and E.**

1. IN several communications presented to the Royal Society, and printed in the Philosophical Transactions, it has been my endeavour to show that the remarkable process effecting the division and subdivision of what is usually termed the "yelk" in the mammiferous ovum, is to be recognised in other cells; and nowhere more distinctly than in certain states of the corpuscles of the blood. In proof of this, I gave the delineations, figs. 1 to 5, along with many others. Figs. 1, 2, 3



represent blood-corpuscles (cells) of the Sparrow. 1. The nucleus single: 2. the nucleus dividing into two parts: 3. this division is complete. Figs. 4, 5 are blood-corpuscles (cells) of the fœtal Ox, three-quarters of an inch in length. 4. The nucleus consists of two discs: 5. the discs have separated, and much increased in size, and they are passing into the state of cells.

2. Such having been my views, it was very satisfactory to meet with the following confirmation of them. In a lecture



delivered a few weeks since at the College of Surgeons, Professor Owen exhibited the sketches 6 to 10, in a thesis by Dr. Bagge†, representing successive stages in the development of the ovum of an intestinal worm; and stated the results of Dr. Bagge's observations. I find the following remarks on this subject in the published lecture of Professor Owen:—"There is a close and interesting analogy between the above phænomena [observed by Dr. Bagge, fig. 6 to 10], which were published in 1841, and some of those communicated by Dr. M. Barry to the Royal Society, in January 1841, and published in the Philosophical Transactions of the same year. The clear central nucleus of the blood-corpuscle is there shown to form two discs‡, which give origin to two cells. We may, likewise,

* Communicated by the Author.

† *De evolutione Strongyli auricularis et Ascaridis acuminatæ viviparorum.* Erlangæ, 1841.

‡ See Phil. Trans. 1841. pl. 18. fig. 37. [and pl. 17. fig. 24.]

discern in the pellucid nucleus of the yolk, dividing and giving origin to two yolk-cells, according to the German author, the hyaline nucleus of Dr. M. Barry*."

3. Professor Rudolph Wagner observed that the size of the blood-corpuscles in the naked Amphibia is "so much the larger, the longer the gills continue in the larval state." Thus the blood-corpuscles are larger in the Newt than in the Frog. He hence conjectured that the Proteus and Siren, because they permanently have both gills and lungs,—being therefore permanently larvæ,—would be found to have the largest blood-corpuscles. In the Proteus he had the opportunity of seeing the idea realized†.—This connexion between the size of the blood-corpuscles and a larval condition of the animal, I believe has not been explained.

4. On first seeing the large cells in the mammiferous ovum‡, I was struck with the resemblance they bore to the corpuscles or cells of the blood in, for instance, the Batrachia; which was also remarked by Dr. Roget on seeing my delineations of the former: and I have since (§ 1, 2) shown them to be perpetuated by the same means. Finding also in the blood of the mammiferous *embryo* corpuscles or cells (figs. 4, 5) like the ordinary blood-corpuscles or cells of the *adult* Batrachia, &c., I conceived that the difference between the condition of the blood-corpuscles in the embryo and in the adult of the same animal, was referable to a difference in the degree of their development as cells§.

5. Now there are facts, I think, which leave little doubt that the blood-corpuscles—not only in the embryo, but at all periods of life—are descended from the two cells constituting the foundation of the new being in the ovum; cells arising out of previously existing cells, by self-division of the nuclei.

6. When tracing the early stages in the formation of the embryo, I showed that, as the cells thus increase in number, they diminish in their size. Have we any proof that this diminution in size ceases in later stages? Is it not rather to be presumed that it continues? and, indeed, does not the difference in size between the corpuscles of adult and fœtal blood render it probable that this progressive diminution in size goes on? If so, the younger the larva is, the larger may be its

* Hunterian Lectures, by Professor Owen, F.R.S., from Notes taken by W. W. Cooper, M.R.C.S. 1843. No. 3. p. 78.

† See Proceedings of the Zoological Society, Nov. 14, 1837.

‡ Researches in Embryology, Second Series. Phil. Trans. 1839. pl. 16. fig. 105½, &c.

§ On the Corpuscles of the Blood, Part II. Phil. Trans. 1841. p. 206.

blood-cells. And as a larval state in the Batrachia, &c. is indicated by a retention of the gills, is it surprising that we find their blood-corpuscles large in proportion to the length of time during which they retain the gills?

7. I cannot doubt that a law of the kind now mentioned—progressive diminution in the size of cells—is *general* in its operation; and if so it may regulate the magnitude of the corpuscles in other blood.

LXXIV. *On an application of the Electrotype Process, in conducting Organic Analysis. By* ROBERT MALLET, *Ph.D.*

To Richard Phillips, Esq.

DEAR SIR,

AN application of the electrotype process has been made by me, which appears of some value to those engaged in the pursuit of organic analysis; I therefore hope a brief notice of it may not be out of place in the Philosophical Magazine. When very high temperatures are required to effect or complete difficult combustions with oxide of copper or chromate of lead, as in the determination of carbon in certain varieties of cast iron (which indeed suggested the application to me), the glass tube is liable to soften and get distorted, though of Bohemian glass, and it has been usual to cover it by lapping a spiral strip of thin copper round the tube. This however is never in close contact, even when cold, and the tube is liable to be broken either in the lapping or during the combustion.

The method I have now to mention as a substitute for this, consists in brushing over the outside of the combustion tube with a very thin coat of Canada balsam and turpentine, dusting it over with fine powder of plumbago which adheres thereto, connecting one end of the combustion tube with a copper wire, and plunging the whole into a cell of sulphate of copper in the common electrotype arrangement. In a few hours the whole exterior of the tube is found covered with a perfect, close and coherent jacket or tube of copper, and may now be at once put into use.

The copper covering adheres so close to the glass tube, and is so completely itself an *air-tight* combustion tube of copper, that should the glass tube crack in the combustion it is of little importance.

The film of Canada balsam between is so indefinitely thin that its decomposition has no injurious effect. A combustion tube of 18 inches long is only increased in weight about $\frac{1}{10}$th of a grain by this coating, when dry (without the plumbago

of course). For the latter, Dutch gold-leaf may be substituted with advantage.

The glass combustion tube is best filled with the oxide of copper, &c. and subject of analysis, before the precipitation of the copper upon it, and the tube is best drawn out to a neck at the end next the kali apparatus, as well as at the remote one, the former being done immediately after the filling; the latter neck is opened on commencing the combustion, and the union with the train of absorbent vessels is made by cork in the usual way, but in inverse order, that is, the first cork is not inserted *into* the combustion tube, but placed *upon* the drawn-out neck, thus

The whole tube from A to B being covered with copper, the passage for the gases is ensured by a sharp blow or two on a table of the combustion tube as usual.

In the methods proposed by Prof. Bunsen of Marburg some time ago, chiefly for the determination of nitrogen by combustion in a hermetically sealed tube, he imbedded the combustion tube in a mould of plaster of Paris to prevent the elastic gases evolved from bursting the tube. The process was difficult and uncertain, but the application of the method above described gives as much facility to the performance of organic analysis by his method as by any other.

This mode of precipitating copper upon glass is also susceptible of many other useful applications in the arrangement of chemical apparatus when heat or pressure is concerned.

I have the honour to be, Sir,

Your obedient Servant,

Dublin. Robert Mallet, *Ph.D.*

LXXV. *Supplementary Note on* Brett's *determination of the Foci of a Conic Section. By* T. S. Davies, *Esq., F.R.S. Lond. and Ed., F.S.A., Royal Military Academy**.

I REFERRED to the paper of M. Brett when discussing a more general problem in the Philosophical Magazine for January last, and I stated that the author had not solved his resulting equations, even as adapted to the confined case which he proposed to consider. The most general form of the problem, however, when confined to the foci, without

* Communicated by the Author.

taking into account the directrix, does, notwithstanding, admit of a short and simple solution; and I think the investigation here given will not be without interest to the mathematical readers of the Magazine,—the discussion of the problem under this aspect leading to a series of equations very similar to those which occurred in my own form of the problem.

PROP.—*A point can always be found in the plane of a line of the second order referred to any angle of ordination, such that its distance from any point of the curve is a rational linear function of the coordinates of that point of the curve.*

Let $a y^2 + b x y + c x^2 + d y + e x + f = 0$ be the curve; α the angle of ordination; $h\,k$ the point to be found: then it is affirmed that h, k, p, q, r can be so determined that for all corresponding values of x and y we shall have

$$(y-k)^2 + 2\,(y-k)\,(x-h)\cos\alpha + (x-h)^2$$

of the form $(py + qx + r)^2$.

For multiply the equation of the curve by λ; then we shall have an identity between the two following expressions:—

$$\lambda a y^2 + \lambda b x y + \lambda c x^2 + \lambda d y + \lambda e x + \lambda f, \text{ and}$$

$$(y-k)^2 + 2\,(y-k)\,(x-h)\cos\alpha + (x-h)^2 - (py + qx + r)^2.$$

Equating the homologous coefficients of these expressions, we get

$$\lambda a = 1 - p^2 \quad \ldots \ldots \quad (1.)$$
$$\lambda b = 2\cos\alpha - p q \quad \ldots \ldots \quad (2.)$$
$$\lambda c = 1 - q^2 \quad \ldots \ldots \quad (3.)$$
$$\lambda d = -\,2\,(k + h\cos\alpha + p r) \quad \ldots \ldots \quad (4.)$$
$$\lambda e = -\,2\,(h + k\cos\alpha + q r) \quad \ldots \ldots \quad (5.)$$
$$\lambda f = h^2 + 2\,h\,k\cos\alpha + k^2 - r^2 \quad \ldots \ldots \quad (6.)$$

We have now to show how the values of h, k, p, q, r, λ may be determined, and to prove that they are real.

From (1, 2, 3.) we get at once

$$p^2 = 1 - \lambda a, \quad 2\,p\,q = 2\cos\alpha - \lambda b, \text{ and } q^2 = 1 - \lambda c.$$

Whence

$$4\,(1-\lambda a)\,(1-\lambda c) = 4\,p^2 q^2 = (2\,p\,q)^2 = (2\cos\alpha - \lambda b)^2;$$

or collecting and arranging the terms in $\frac{1}{\lambda}$, we have

$$4\sin^2\alpha \,.\, \frac{1}{\lambda^2} - 4\,(a - b\cos\alpha + c)\,.\,\frac{1}{\lambda} = b^2 - 4\,a\,c. \qquad (7.)$$

Now, putting as in my former paper, page 26, Q and R for their values, we have

$$\frac{1}{\lambda} = \frac{Q+R}{2\sin^2\alpha}, \text{ or } \lambda = \frac{2\,(R-Q)}{b^2 - 4\,a\,c}; \quad \ldots \ldots \quad (8.)$$

and λ is the reciprocal of the u^2 in the solution before referred to.

Using the same notation as before, we get from (1, 3.),

$$p^2 = 1 - \lambda a = 1 - \frac{2\,a \sin^2 \alpha}{Q + R} = \frac{H + R}{Q + R}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

$$q^2 = 1 - \lambda c = 1 - \frac{2\,c \sin^2 \alpha}{Q + R} = \frac{K + R}{Q + R}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (10.)$$

In extracting these roots to obtain p and q, they must be taken with the same or different signs to fulfil equation (2.); and as they may under this restriction have two values, there are two different linear functions, and hence two points determinable, which will fulfil the conditions.

Again, as in the solution already referred to, h and k can be found from (4, 5, 6.); and the general forms being the same, the values will be real, as they were proved to be in that place.

Lastly, the value of r may be found from either of the equations (4, 5, 6.); and as it is of the first degree in those, it is evidently real. However, to obtain a symmetrical form it must be actually obtained from (6.), or from some symmetrical combinations of (4, 5.).

Royal Military Academy, April 7, 1843.

LXXVI. *On a double Rainbow.* *By* Henry Bowman, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

On Tuesday, March 21st, occurred here, between half-past 2 o'clock and a quarter past 3 in the afternoon, a thunder-storm, which, though by no means severe, continued for a considerable time. The lightning was distant upwards of a mile, and the thunder was heard in short detached peals of nearly uniform duration, and almost at regular intervals, and had so little the character of ordinary thunder, that some time elapsed ere I recognized it to be such. It resembled more the alternate opening and closing of a window in an adjoining room.

But the most remarkable phænomenon attending this storm was a double rainbow, which appeared during and after a smart thunder-shower about 3 o'clock. The primary bow, which appeared some time before the secondary, was rather vivid, but the latter soon vied with it, until the two appeared with nearly equal brillancy.

The arcs seemed to reach the horizon at both extremities, and to occupy perhaps 120° of a circle. Within the primary bow, I counted two or three reflexions or supplementary bows, consisting chiefly of red and green rays. The bows were seen against a dense mass of cloud, of a fine neutral tint, and what

strongly attracted my attention was, that the intermediate space between the two bows was of a different shade, being more the colour of Indian ink than neutral tint, and was considerably deeper in tone than the portion of cloud either within the primary or without the secondary bow. The whole together had the effect of a broad circular riband, bordered on each side with the prismatic colours. The alteration in the tint of the cloud was so perfectly coincident with both the bows, as to leave no doubt of the necessary connexion between the two phænomena.

I cannot suppose it to have been an accidental accumulation of cloud in that particular region, but imagine it to have some connexion with the phænomena of light and reflexion or refraction. This singular effect continued nearly as long as the rainbows were visible, viz. about 4 or 5 minutes.

The masses of cloud seemed to accumulate towards the centre of the circle, and also at some distance on the outside of the secondary bow, though this probably was only an accidental coincidence, but in effect it was as though the clouds had been partially withdrawn from both sides of the double band, and collected in the space between the two bows.

Victoria Park, Manchester, April 15, 1843.

LXXVII. *On the Appearances and Relative Positions of the Rocks and Veins which form the Opposite Walls of Cross-Veins. By* W. J. Henwood, *C.E., F.R.S., F.G.S., M. Inst. C.E., Member of the Geological Society of France, of the Royal Geological Society of Cornwall, &c.*

[Continued from p. 384.]

II. LET us now apply to the intersections of two or more lodes, or other veins, by the same cross-vein, the same laws which have been assumed to prevail when there is but a single intersection.

(1.) In the event of a horizontal movement, any number of veins contained in the mass of rock displaced, should be always heaved to the same distance in every instance, and for the same extent at all levels: but this result never occurs (*a*, *d*, *e*, *f*, *g*, *h*, *i*, *m*, &c.).

But, although a horizontal motion will not accurately apply, as its results will not agree with the order observed, it will, nevertheless, produce effects bearing a nearer resemblance to the facts than those following motion in any other direction. In short, where two or three lodes are intersected by the same cross-vein, 70·2 per cent. of the total number are

heaved in the same direction; whilst less than one per cent. is heaved to the opposite side. On the other hand, of the cross-veins which traverse two or three lodes each, 8·8 per cent. simply intersect one of the lodes, and heave the rest. And in almost innumerable instances, as at Dolcoath, Cook's kitchen, the United and Consolidated Mines, Cardrew Downs, &c. (*s*, *c*, *i*, *e*, &c.), the same cross-veins traverse both elvan-courses and lodes; and whilst the former are simply intersected, the latter, on either side of them, are heaved.

Notwithstanding the greater number of lodes heaved by the same cross-veins is heaved in the same directions, it seldom or never happens that both, or all of them are heaved the same distance; which is utterly incompatible with the supposition of their having originated in horizontal movements (*d*, *e*, *f*, *g*, *h*, *i*, *j*, *k*, *l*, &c.).

(2.) If two lodes, with opposite inclinations, be transversely fractured, and one of the segments vertically elevated, whilst the other remains unmoved, the lodes must be heaved in opposite directions at all levels. This is a physical necessity, and it is utterly impossible that any other result could follow such conditions.

Let A B (Pl. IV. fig. 9, 10) be the surface of a given tract in its original position, A^1 B^1 that of another which formerly stood at the same level with, and was united to, A B, but which has since been elevated vertically, and which now stands beyond A B. Y Z is a transverse section perpendicular, or nearly so, to A B, and Y^1 Z^1 a similar one, in like circumstances with regard to A^1 B^1.

Let also a, a^1 represent the superficial parts of one vein or lode, and b, b^1 its deeper portion; c, c^1 the upper parts of another vein, and d, d^1 the lower. The two portions respectively of either vein must be presumed to have been originally united and continuous, but to have been severed at the cross-vein w x, fig. 10. The inclinations of the two veins, it must be observed, are in opposite directions.

We will suppose the original state of things restored; A B, and A^1 B^1 on the same level, and a, a^1 and c, c^1 respectively united. Let A^1 B^1 be now perpendicularly elevated from p to o. This movement will bring a portion of the lower parts of the lodes, where by their dips towards each other they are closer together, opposite the upper portion where they are further apart.

Such a motion will evidently break the continuity of both; and thus, if the portion above p, on the line w x, fig. 10, were removed by denudation or any other cause, the surface then presented would be that depicted in the plan (fig. 10). In

short, one lode would be heaved towards the right-hand, as *a*, *a*¹, and the other to the left, as *c*, *c*¹. These results are not mere probabilities, but physical necessities, which, and which alone, must inevitably follow a vertical elevation of portions of lodes having opposite dips.

In this case, too, there can never be a simple intersection unless the dip is reversed: and not even then, unless the elevation be to such an extent as will directly oppose to each other portions which have these opposite inclinations.

The examples, therefore, in which lodes with opposite inclinations are heaved in the same directions (*a*, *b*, *c*, *d*, *e*, *f*, *g*, &c.), as well as those in which there are heaves at some levels, and simple intersections, without reversed dips at others (*b*, *n*, *q*), are equally unaccounted for on this hypothesis. The insufficiency of its general application is also most decided in all cases where the cross-vein heaves the lodes, but simply intersects the elvan-courses which lie between them (*e*, *f*, *g*, *i*, *p*, &c.). These contradictions between fact and theory nothing can reconcile.

(3.) Let us now take an example, in which, as in the last, there are two veins having opposite inclinations, with a third vein between them which dips in the same direction as one of the others, but at a different angle. Let the assumed motion be parallel to the dip of the latter.

I put this as a case which, in some respects at least, satisfies the conditions left unsolved by the last, viz. two lodes, having opposite dips, being heaved in the same direction, whilst a third (an elvan-course) occurring between them is simply intersected.

Let A B (Pl. IV. fig. 11, 12), as in the last case (2.), be the unmoved surface, and A^1 B^1 the elevated one; Y Z the unmoved transverse section, and Y^1 Z^1 that which has been raised; *a*, *a*¹, and *c*, *c*¹ the superficial parts of the same lodes, which, when at the same level, had been respectively united; and *e*, *e*¹ the upper portions of the elvan-course, once united, and still unheaved: *b*, *b*¹, *d*, *d*¹, and *f*, *f*¹ the deeper parts of the three veins respectively.

Now if A^1 B^1 be further from us than A B, and if we suppose it to be elevated on the line *s r*, parallel to the dip of *f*, *f*¹, and the superficial portion above A B removed as before described, the plan, fig. 12, will present an idea of the new state of things; the cross-vein, *w x*, having been formed during the elevatory action.

w x will break the continuity of *a*, *a*¹, and *c*, *c*¹, and heave both of them; whilst as the motion upward, on the line *s r*, parallel to *f*¹, will keep *e* and *e*¹ still in contact at all levels,

and the dip remaining the same, this third vein will be simply intersected.

Now, as *e* dips in the same direction as *c*, although not at the same angle, whilst *a* has an opposite inclination, it is evident that as the movement is parallel to f^1, that vein will suffer no heave; and that as the dip of *c* forms a smaller angle with f^1 than (*b*) the inclination of *a* does, the heave of *c* will be of smaller extent than that of *a*.

The line of motion (*s r*) being oblique, and flatter than d, d^1, will in this instance occasion the heave of *c* to be towards the right-hand as well as that of *a*, instead of their being in opposite directions, as would be the effect of a vertical motion.

This agrees with the facts much better than the hypothesis of a vertical movement: but we can arrive at no positive proof of the sufficiency or insufficiency of its application to a full explanation of the observed phænomena of heaves, unless by comparing the actual motions which would be imparted to a^1 and c^1, in examples where it is supposed to have been parallel to the dip of the unheaved vein.

If the same cross-vein traverse several veins, and simply intersects one of them whilst it heaves all the others, and if these displacements have been occasioned by an elevation of one side of the cross-vein, or a subsidence of the other, it is evident that the direction of the movement must have been parallel to the dip of the vein which is not heaved; and what may have been the extent of the motion is determined by the distance of movement, in the before-named direction, requisite to produce heaves of the extent observed. In other words, the direction of the motion is from *s* to *r*, fig. 11: and, in order to obtain the heaves a, b^1 and c, d^1, fig. 11, or a, a^1 and c, c^1, fig. 12, its extent must also be the distance *s r*. Thus the unheaved vein indicates the direction, and the heaved one the extent of the motion.

We will now compare the observed phænomena with this test, in order to see whether a movement parallel in direction to the dip (f, f^1) of the vein simply intersected, and of the distance *s r*, whilst it produces the observed heave a, a^1, fig. 12, will, by a motion similar both in direction and extent, occasion at c, c^1 a heave which shall correspond with observation in every particular; or, on the other hand, whether a motion, in the same direction, and in distance sufficient for the production of the heave c, c^1, will at the same time heave a, a^1 in such a direction, and to such a distance, as the actual fact presents.

(A.) At the Consolidated Mines (Table LXII.), Tiddy's or Paul's cross-course intersects without heaving an elvan-

course, which dips N.E. 50°, and also Glover's lode, which dips S. 50°—80°; whilst it heaves Paul's lode, which dips N.W. 50°—70°, from 15 to 18 feet at different levels, and Michell's lode, which dips S. 80°, 3 feet, and both of them towards the right-hand (*i*).

We have here two veins, the elvan and Glover's lode, dipping in opposite directions, and both simply intersected; and two others which have also opposite inclinations, and are, nevertheless, heaved in the same direction, but to unequal distances.

Now a horizontal motion (1.) would have heaved them all the same distance, and in the same direction; and a vertical movement (2.) would have heaved Glover's and Michell's lodes in one direction, and the elvan-course and Paul's lode in the opposite: whilst a motion on the line of inclination of Glover's lode would have occasioned a heave of the elvan-course, as well as of Paul's and Michell's lodes; on the other hand, if the line of elevation or subsidence had coincided with the dip of the elvan-course, it must inevitably have heaved Glover's lode (which has an opposite dip to the elvan), together with Michell's and Paul's lodes.

We have thus, within a very few fathoms, two sets of facts equally contradictory to each other, and to all the directions of motion we have yet assumed: nor does any circumstance indicate that any other motion could be substituted with greater probability of overcoming the difficulty.

(B.) At Cardrew Downs, two elvan-courses dip W. 50°—60°: both of them are intersected, both by the Little and the Great flucans, but they are not heaved by either. The south lode dips N. 60°—80°, and the north lode dips S. 68°—82°. The Little flucan heaves the former 24 feet at 50, and 60 fathoms deep, and 30 feet at 100 fathoms; and the latter 15 feet at 50, 70, and 80 fathoms deep, and 12 feet at 60 fathoms. All the heaves are towards the right-hand.

Now if the heaves had been consequences of horizontal motion (1.), not only would all of them have been of the same extent, but the elvan-courses would have been heaved as well as the lodes. If the motion had been a vertical one (2.), the lodes would have been heaved in opposite directions, and both the elvans would have been heaved also. As, however, the elvans are not heaved, any motion must have been parallel to their inclination (*s r*, Pl. IV. fig. 11). Now, in order to produce a heave of 24 feet at the south lode by an elevation parallel to the dip of the elvan, a movement of from 25 to 30 feet in extent is requisite; whilst a motion, similar in direction and distance, would produce a heave of between 5 and 6 feet in the north lode, whereas the observed heave is 15 feet. This

discrepancy between the computed and observed results is surely inconsistent with accidental fluctuation (*e*).

(C.) At Wheal Unity Wood the Great elvan-course dips N. 40°—50°: Trefusis lode also dips N. 66°—82°; the Little Ore lode likewise dips N. 52°—60°; and Pits-an-vollar lode inclines in the same direction 60°—70°. The flucan simply intersects the elvan-course; whilst it heaves Trefusis lode 36 feet at 26 fathoms deep, and 30 feet at 36 fathoms; the Little Ore lode 12 feet at 60 fathoms deep, and Pits-an-vollar lode 21 feet at the same level. In all these cases the heaves are toward the left-hand.

The elevatory action, if any, must of course have been parallel to the dip of the simply-intersected vein,—the elvan. Now in order that a motion in this direction should produce a heave of 36 feet in Trefusis lode at 26 fathoms deep, the extent of the movement must be from 85 to 90 feet; whilst to produce a heave of 30 feet, in the same lode, at 36 fathoms, a motion of only about 55 in the same direction would be requisite. But an elevation of 90 feet, in a direction parallel to the underlie of the elvan, would produce a heave of 21 feet in the Little Ore lode at 60 fathoms deep, and one of 55 feet would occasion a heave of 12 feet; whereas the same quantities of motion, acting in the like direction on Pits-an-vollar lode at 60 fathoms, would respectively cause heaves of 45 and 27 feet.

Thus, in order to the production of the observed phænomena, we have at Trefusis lode these scarcely compatible conditions:—either the direction of the motion was the same at both, and its extent different, or the extent of motion was the same at both, and its direction different.

The same extent and direction of motion which will produce the heave observed in Trefusis lode at 36 fathoms deep, will also give the real heave of the Little Ore lode at 60 fathoms; whilst the extent of motion necessary to obtain the heave of the former at 26 fathoms deep, will occasion a heave of 21 feet in the latter; being an excess of 9 feet over the observed distance.

If the motion at Pits-an-vollar lode at 60 fathoms deep were of similar direction and extent to that assumed to have acted on Trefusis lode at 26 fathoms deep, the heave would have been 45 feet; or, if the same as that at 36 fathoms, it would have been 27 feet. Now the observed heave is 21 feet; and in order to obtain this with a motion parallel to the unheaved vein (the elvan), an elevation of about 40 feet would have sufficed: and such a degree of motion, at the same depth, would have occasioned a heave of about 9 or 10 feet in the Little Ore lode, instead of the observed distance of 12 feet (*f*).

In the foregoing examples the elvan-courses are the simply-intersected veins: it may now be desirable to examine a few in which the unheaved veins are lodes.

(D.) At Fowey Consols, at 90 fathoms deep, the cross-course simply intersects Bone's and Jeffery's lodes; but at 95 fathoms, where Crosspark lode is in two veins, it heaves one of them 9, and the other 15 feet towards the right-hand, and at 200 fathoms, where there is only one lode, it is heaved 2 feet in the same direction (*q*).

The vein of Crosspark lode, which in dip most closely approximates to the unheaved lodes (Bone's and Jeffery's), is heaved further, whilst the other vein, which differs most from them in inclination, is heaved the smaller distance; and yet the difference in the inclinations of these veins is only about three degrees. Now, taking their mean dip and the direction of the movement to be parallel to Bone's and Jeffery's lodes, the extent of motion requisite to produce a heave of 9 feet would be about 26 fathoms; but, to obtain a heave of 15 feet, an elevation of nearly 39 fathoms in the same direction would be requisite. Applying these directions and extents of movement to the lode at 200 fathoms deep, where it has a flatter inclination, a motion of 26 fathoms would cause a heave of 30 feet, and one of 39 fathoms a heave of 39 feet: the actual heave, however, is only 2 feet.

Here, then, we have, in one case, that lode heaved furthest which, according to the theory, ought to have been heaved the least; and in the other a heave of only 2 feet, whereas the hypothetical motion demands one of 30 or 39 feet.

For the present I pass the consideration of the heave of this same cross-course by Williams's lode, as it will be again necessary to advert to it (V.–3).

(E.) At Stray Park the two lodes have opposite dips, and both are intersected by the same cross-courses. Now a horizontal motion (1.) would have caused similar heaves in both lodes at all levels. A vertical motion (2.) would have caused them to be heaved at all levels, and everywhere in opposite directions. Any movement parallel to the dip of one of them (3.), would, for the most part, have simply intersected, or at any rate but slightly heaved it*; whilst its effects on the other lode would have been much greater. Lastly, a rotatory motion (4.) would have produced results constantly increasing in magnitude as the spot was more remote from the neutral point or centre of motion.

* In consequence of the lode's dip not being perfectly uniform.

Let us compare these demands of theory with the facts observed :—At

108 fms. deep,		both lodes are simply intersected by one cross-course.
162	...	both lodes are heaved to the left-hand by one cross-course, and to the right by the other.
178 } 188 }	...	both lodes are simply intersected by both cross-courses.
198	...	both lodes are simply intersected by one cross-course.

Similar results, at the same levels in both lodes, whether intersected by one or both of the cross-courses, appear in every instance; except at 146 fathoms deep, and there one lode is simply intersected, whilst the other is heaved towards the left-hand.

These facts seem utterly irreconcilable with the result of motion in any one single direction that can possibly be assumed.

But we have not always simple intersections to indicate the directions in which any elevatory forces must have acted. In the absence of the guidance such examples afford, it will be requisite to ascertain whether the observed conditions may or may not be fulfilled by motions which are neither horizontal, vertical, nor yet coincident with the dips of any of the veins, but oblique to them all.

Very little consideration will convince us that heaves in the same direction, but of very different distances, may in this manner be effected by motion of the same extent and direction; as it will act on the lodes according to the coincidence or obliquity between their dips and the direction of the movement. Such a state of things, arbitrarily selected in order to suit the circumstances, may often be applied to the heaves of two lodes by the same cross-vein, when in the same direction, but of unequal distances. But the sufficiency or insufficiency of this hypothesis is tested by its affording, or not, in a third, fourth, or fifth intersection by the same cross-vein, the same coincidence between theory and fact as were obtained in the first and second.

(F.) At Wheal Prudence an elvan-course dips N. 45°, and appears in the face of the cliff: the north lode dips N. 68°—80°, and the south lode dips S. 68°—85°. The cross-course heaves them all towards the right-hand,—the elvan-course 30 feet, the north lode 9 feet, and the south lode distances differing at different levels, and varying between 18 and 30 feet (*g.*)

A horizontal motion would have heaved all the veins to the same distance; a vertical movement would have heaved the south lode in a direction opposite to the others; and a motion coincident with the dip of either would have occasioned a

simple intersection of that vein, and heaves in the other two; all which results are opposed to the facts.

Now an elevation on a line which might dip northwards about 87° from the horizon, would, by an equal extent of motion (about 34 feet in both cases), occasion a heave of 30 feet in the elvan-course, and one of 9 feet in the north lode, both of which accord with observation. When, however, we apply the same extent and direction of motion to the south lode, the discordance between fact and theory becomes most obvious, and is shown below:—

Depth.	Calculated dist.	Observed dist.
80 fms.	14 feet	30 feet.
92 ...	14 ...	18 ...
100 ...	12 ...	30 ...
110 ...	12 ...	21 ...

The failure of this attempt at theoretical explanation can scarcely be rendered more striking.

(G.) At Polladras Downs all the lodes dip N.:—the Bor lode, 56°—80°; Bissa lode, 50°—82°; Pressure north lode, 56°—74°; Pressure south lode, 56°—82°; and Richards's lode, 72°—76°. The flucan heaves them all towards the right-hand (*o*).

The inclination of a line on which a given extent of motion will produce the greatest number of results approximating to the observed facts, is about 85° towards the south (from the horizon); and the extent of the motion required is about 17 or 18 fathoms. The differences between the calculated and observed distances of the heaves are as follow:—

	Depths.	Calculated dist.	Observed dist.
Richards's lode	13 fms.	42 feet	42 feet.
Pressure S. lode	33 ...	42 ...	42 ...
...	43 ...	40 ...	42 ...
...	53 ...	43 ...	72 ...
...	83 ...	60 ...	42 ...
Pressure N. lode	33 ...	45 ...	24 ...
...	83 ...	78 ...	72 ...
Bissa lode	43 ...	84 ...	84 ...
Bor lode	73 ...	42 ...	12 ...
...	83 ...	42 ...	12 ...
...	93 ...	53 ...	18 ...
...	103 ...	63 ...	24 ...

Thus, of 12 heaves, the assumed motions coincide in 3 instances, and differ from the observed results in the remaining 9.

If, on the other hand, preserving the direction of the mo-

tion, we assume its extent in each case to be equal to the production of the observed results, it will be as follows:—

	Depth.	Extent of elevation.
Richards's lode	13 fms.	17·5 fms.
Pressure S. lode	33 ...	17·5 ...
...	43 ...	20·0 ...
...	53 ...	32·0 ...
...	83 ...	14·0 ...
Pressure N. lode	33 ...	8·6 ...
...	83 ...	16·0 ...
Bissa lode	43 ...	17·5 ...
Bor lode	73 ...	5·6 ...
...	83 ...	6·0 ...
...	93 ...	5·6 ...
...	103 ...	6·2 ...

The differences here are certainly not less than in the first series of comparisons.

Let us still preserve the direction of the movement, but see the varying extents of it requisite to produce heaves of the same magnitude at all the points of observation. Let the extent of the heave be taken at 42 feet, which is about the mean in this mine:—

	Depth.	Extent of elevation.
Richards's lode	13 fms.	17·5 fms.
Pressure S. lode	33 ...	17·5 ...
...	43 ...	20·0 ...
...	53 ...	18·0 ...
...	83 ...	12·2 ...
Pressure N. lode	33 ...	16·5 ...
...	83 ...	9·3 ...
Bissa lode	43 ...	9·5 ...
Bor lode	73 ...	17·5 ...
...	83 ...	17·5 ...
...	93 ...	14·0 ...
...	103 ...	11·3 ...

At Polberrow (*h*) and the United Mines (*m*) similar differences occur.

The number of examples in which several veins are intersected by the same cross-vein is but few; in all of them, however, it will be found that no motion in one direction, and of the same extent, will restore the continuity of every vein so intersected:—and I must here repeat, that the difficulty is in the connexion which subsists between them.

(4.) A curvilinear motion is the only one now remaining for comparison with the facts observed. In order to ascertain

the effect of such a motion, it is necessary to have particulars at different distances, on both sides of the presumed centre. These details may be either the directions and distances of heaves of different lodes at the same level, or of the same lode at several levels.

It has been already seen (4.) that a curvilinear motion will only apply to heaves which progressively increase in magnitude, or to those which are in opposite directions in the same vein at different depths, or at the same levels, if the veins be different.

No example of a progressive increase in the extent of a heave has been recorded.

At the United Mines (*m*), Skues's flucan heaves 7 lodes, which occur in regular sequence towards the right-hand; and 7 others, which also follow each other in succession, towards the left-hand. Of the first seven, 2 dip towards the north, 1 is perpendicular, and 4 dip south; of the second seven, 1 dips north and 6 dip south.

As all the lodes on one direction from a given point are heaved towards the right-hand, and all those on the other towards the left, if this had been occasioned by a rotatory motion, the lodes furthest from the neutral point, axis, or centre of motion, would have suffered a movement progressively increasing in extent: consequently the magnitude of the heaves ought to increase progressively as they are situated further from this axis, or point round which the motion has taken place. Now the four right-hand heaves which are furthest from this point are respectively 6, 6, 2, and 5 feet in extent, and the four left-hand heaves, similarly situated, are two of 6 feet and two of 18 feet each; whilst the lode nearest to the point of division on one side is heaved 7 feet, and the corresponding one on the other 18 feet.

Thus the lodes nearest to the neutral point are heaved further than those which are most distant—facts in direct opposition to the theory.

III. When the same lode is intersected by two cross-veins, namely, a, a^1, a^2, by $w\,x$ and $y\,z$ (Pl. IV. fig. 13), if the portion a^1 contained between them be the only one moved, any motion, excepting one parallel to the dip of the lode a, a^1, a^2, must necessarily produce two heaves in opposite directions; for no motion of a^1 can possibly occasion a simple intersection by one cross-vein and a heave by the other.

Of 55 lodes, each traversed by two cross-veins, producing 110 intersections, there are 90 heaves. Now this hypothesis would require that every lode should be heaved in opposite directions by the two cross-veins intersecting it; but this is

the case with only 42 of them; and the remaining 48 are heaved in similar directions. Thus 46·6 per cent. countenance the supposed law, and 53·4 per cent. contradict it; and consequently the evidence against it preponderates.

Suppose, then, a series of heaves of the same lode, in the same direction, by different cross-veins: these must therefore be occasioned by successive steps; or the masses of rock which contain it must be supposed to have suffered a series of elevations each greater than the preceding; the first step being next to the unmoved rock. Thus, at Polladras Downs (*o*), a series of elevations, successively increasing as we go westward, will afford that superficial explanation which we have already seen (G.) is contradicted by a more minute investigation. A similar succession of movements will also apply to the Old lode at Poldory (United Mines) (*i m*), and a like explanation will apply to Wheal Falmouth.

At Cook's-kitchen (*c*), a system of elevations augmenting as they are followed eastward will produce the same apparent coincidences; and similar ones are also found in Wheal Jewel, Wheal Friendship, and in the heaves of the south lode at Cardrew Downs (*e*, B).

But when the heaves by successive cross-veins are in opposite directions, certain masses of rock must be heaved, and others, interposed amongst them, remain in their original positions; as, for example, at Herland (*n*), where this assumption requires that the portion contained between the Halfpenny flucan and Chambers's western flucan shall not have been moved; whilst, at every cross-vein west of the former, there shall have been, successively, an elevation more considerable as we go westward, and at every cross-vein east of the latter, a similar one, still increasing in magnitude, as we go eastward.

At Wheal Vor (*l*), we may imagine the heaves by the two portions of the eastern cross-course to have been attended by depressions westward; but then we must require that the part between Woolf's and Carleen cross-courses shall have remained stationary, whilst the part west of the latter has also subsided. But it must still be remembered that these movements will produce only an apparent agreement, and explain only one set of phænomena; for when we compare them with the distances of the heaves, the resemblance vanishes.

These movements in opposite directions, and stationary masses interposed between others which have suffered contradictory motions, certainly require a liberal indulgence of imagination.

But whether the two cross-veins be wholly separate and

distinct ones, or whether they may be branches of the same which unite and form one vein at other places, the physical necessity that their heaves of the same lode must be in opposite directions will still exist, provided the only mass of rock moved is that contained between them; for theory makes the extent of the heaves dependent on the inclination of the lode and the distance of the motion, as we have already so often seen.

Now the spaces at present occupied by the cross-veins are supposed to have been fissures originating in the movements which produced the heaves of lodes, although their ingredients may have been introduced at a subsequent period.

At Wheal Union (*k*) we have every particular necessary to determine whether the same degree and direction of motion will produce fissures of the same width as the branches of the cross-vein, and at the same time heave the lode to the extent, and in the directions, observed at the different levels.

At 22 fathoms deep the trawn heaves Wheal Gwens lode 36 feet, and at 30 fathoms 25 feet; but below this level it divides into three veins, of which one heaves the lode 1·5 foot; the second, 8 feet; and the third, 24 feet: in all cases towards the right-hand. At the upper edge of the wedge-shaped mass of rock included between the eastern and middle branches of the trawn, the former of them is 6 inches and the latter 3 feet wide.

Now in order to the production of fissures of these respective dimensions by the subsidence of this included mass of rock, in a direction op, (Pl. IV. fig. 14) parallel to a line uniting its present vertex with the point of the cavity at o, which it must, on this assumption, be presumed to have originally filled, the extent of the subsidence must have been about 3·75 feet.

Again, in order that a subsidence of this wedge-shaped portion, to that extent, should produce at 43 fathoms deep the observed heave of 1·5 foot in the lode by the eastern branch of the trawn, the direction of the fall must be in a line which dips northward about 72° from the horizon.

But this amount and direction of motion, instead of the observed results of 25 and 36 feet respectively at 30 and 22 fathoms deep, will only produce heaves of 1·5 foot and 1·3 foot.

But if the direction and extent of the motion be the same in all cases, the mechanical effects will be the same whatever is the width of the cross-vein, or whether it consists of a single vein, or of several.

Let us, then, assume a movement in the direction already named, dipping northward 72°, but of a sufficient magnitude

to produce a heave of 36 feet at 22 fathoms deep: the extent of such a motion must be about 17·5 fathoms, and the following is a comparison of the calculation from such data with the facts observed:—

	Distance of heave.	
Depth.	Calculated.	Observed.
22 fms.	36 feet.	36·0 feet.
30 ...	40 ...	25·0 ...
43 ...	47 ...	33·5 ...

But theory requires that the branches of the western trawn shall be attended by motions successively of greater magnitude as we go westward. But as the two western branches have a westerly dip, any subsidence of the mass (horse) of granite between them must close and obliterate any openings or fissures with a very much smaller extent of motion than is requisite for the production of the heaves. These theoretical demands are in direct contradiction to each other; and are also inconsistent with the prevailing opinions*, that the movements which caused the heaves were contemporaneous with the origin of the fissures now occupied by the cross-veins.

At 140 fathoms deep, in Wheal Vor, the eastern cross-course divides into two veins, which diverge as they descend: the lode passes through the wedge-shaped piece of rock included between them, and is heaved by both of them towards the left-hand. The fracture here theoretically assumed would have permitted this included portion to have subsided. Now, at the point of divergence, at 140 fathoms deep, the eastern branch of the cross-course is 6 inches, and the western 3 feet, in breadth; and the extent which the wedge-shaped mass must have subsided from the position it filled before the fracture, in order that it might leave a vacuity of the requisite dimensions, would be about 13·5 feet.

In order that a motion of 13·5 feet in extent should produce a heave of the observed distance of 9 feet at 150 fathoms deep, the direction of the movement should be on a line dipping southward about 57° from the horizon. The following is a comparison of the respective distances of the heaves, which, according to computation, will be produced by a movement of this extent and direction, with the heaves observed:—

	Extent of heave.	
Depth.	Calculated.	Observed.
150 fms.	9 feet.	9 feet.
170 ...	10 ...	5 ...
180 ...	9 ...	7 ...

Again, the following shows the distance of the movement

* Mr. De la Beche, Report, p. 297.

necessary to occasion the heaves observed at the different points:—

Depth.	Heaves observed.	Extent of motion required.
150 fms.	9 feet.	13·5 feet.
170 ...	5 ...	8·0 ...
180 ...	7 ...	10·5 ...

Trifling as the actual differences are, their proportion to the total amount of the heaves gives them an importance they would not have otherwise possessed.

Here, too, the hypothesis which requires the subsidence of the portions of rock included between the branches of the cross-course, and also of that between this cross-course and Woolf's, encounters the same incompatible conditions, which require for the production of the observed heaves such an extent of motion as would have quite obliterated every trace of the cross-course, and even closed every fissure which other causes might have produced in the same position.

IV. The configuration of the parts of the same lodes in contact with opposite sides of cross-veins.

The impossibility, with very few exceptions, of restoring the continuity of the lodes heaved by the same cross-vein, by any one single motion of equal extent in all the cases, naturally leads us to examine the configuration of the portions of the same lode on opposite sides of the cross-vein, that we may ascertain whether they present such a similarity of outline as may furnish an argument in this inquiry.

For, had they been originally connected and continuous, and subsequently separated by a transverse fracture accompanied by an elevation or depression of one of the severed portions, on delineating the line of dip of both portions where they are in contact with the cross-vein, we should expect to find that they had a perfectly or nearly similar outline; although the direction of any movement they had undergone might not allow the corresponding parts to be found at the same levels.

The tables (1—98.) contain sufficient details of the inclinations of all the veins described to furnish us with a close approximation to their true dips. Now if the underlie of a lode were delineated, and, at successive levels, the observed distances of the heaves were marked, and these fixed points were united with each other by lines, the two lines (of inclination) thus obtained ought to present an uniformity of contour: and the parts corresponding in figure should be found at the same or at different levels, according to the direction of the motion which had affected one or the other portion.

Pl. IV., figs. 15, 16, 17, 18, 19, 20, present these com-

parisons; and it will be difficult, if not impossible, to imagine that lines so utterly dissimilar could ever have been united and continuous at all parts of their descent, and fractured after they had become perfectly hardened.

V. The evidence of the relative ages of veins deduced from their intersections.

It has long* been strenuously maintained, that when one vein intersects another, the vein intersected is older than that which intersects it: but the district under consideration affords so many exceptions to the generality of this law, that, in Cornwall at least, it must be received with some limitation†.

Whilst the mineral composition of the containing rocks remains the same, the general character of any given vein, whether it be a lode or a cross-vein, is usually so uniform, that there can be but little doubt that similar portions of the same vein are of the same ages, although they may be situate at some distance from each other, either horizontally or vertically.

(1.) At Ting Tang and Wheal Friendship, strings of vitreous copper ore and copper pyrites pass through the cross-veins and connect together the same ores existing in the lode on either side; and at East Wheal Rose lead ore is found under circumstances precisely similar.

(2.) At the Consolidated Mines‡, two quartz rocks, which fitted exactly into each other, were found at the two ends of a lode heaved by a flucan. The flucan, in the intermediate distance of 7 feet, presented a horizontal open space perfectly corresponding with the contour of the rocks in the ends of the lode.

(3.) At Fowey Consols (*q*), Tincroft (*r*), and Duffield (*p*), the cross-veins which heave some of the lodes are themselves heaved by other lodes exactly similar in composition; and, in short, possess all the characters common to the lodes which are heaved.

(3 *a*.) At Polgooth (*ff*) two lodes are heaved by one elvan-course, whilst one of the same lodes intersects another elvan-course in every respect similar to that by which it is itself intersected.

* Dr. Borlase, Nat. Hist. of Cornwall (2nd edit., 1758), p. 152; Mr. Pryce, *Mineral. Cornub.* (1778), pp. 82–101; M. Werner, Theory of Mineral Veins (1791), p. 51; Mr. Thomas, Report (1819), p. 21; Mr. Carne, Cornwall Geol. Trans. (1819), ii. p. 123; Mr. Hawkins, ibid. p. 227; Professor Phillips, Geology, Cab. Cyclop. (1839, No. cxi.), ii. p. 136; Mr. De la Beche, Report (1839), p. 353.

† "I think it can be shown, that the mere fact of intersection ought not, apart from other considerations, to be taken as evidence of the relative ages of veins." Mr. R. W. Fox, Report of the Royal Cornwall Polytech. Soc. (1836), p. 42.

‡ My own paper, Cornwall Geol. Trans., iii. p. 329.

(3 *b.*) At Wheal Bellon one of the tin lodes heaves two others, but is itself also heaved by another tin lode. The ingredients of all four lodes are exactly alike.

(4.) At Great Work (*y*), Tincroft (*r*), the Morvah and Zennor Mines (*x*), West Pink (*w*), and Dolcoath (*s*), the veins which are intersected at some levels, at other levels intersect the very same veins by which they had been traversed.

In the foregoing examination of the general facts we have seen that every attempt has failed to restore the continuity of any series of veins traversed by the same cross-veins; since any one motion, uniform in direction and extent, will be followed by greater discordances in the relative positions of the portions on opposite sides, than those which at present subsist. The parts, also, in contact with the opposite sides of the cross-veins have scarcely the faintest resemblance in contour to each other, whether the comparison be made at the same or at different levels.

It has also been shown that many heaves are occasioned by cross-veins which first appear at considerable depths below the surface (*b b*); others by cross-veins which dwindle as they descend, and ultimately die away (*c c*); and that some of the cross-veins seem peculiar, or confined to certain lodes, and do not extend to parallel ones, however near (*d d*); whilst others appear only at certain levels on a single lode, and disappear upwards, downwards, and at either end (*e e*).

It has also been observed that there are breaks in the continuity of some lodes, which are re-discovered either towards the right- or left-hand, although neither cross-vein nor joint of the rock intervenes (*g g*).

The utter impossibility of re-producing continuity in all the lodes supposed to have been heaved by any one motion, uniform both in direction and extent, at all spots on the same cross-vein, seems to show decisively that the heaves can never have originated in movement of that simple and general character; even making the utmost allowance for minor fractures and modifications by other local causes.

On the other hand, the existence of heaves where so many conditions conclusively prove the physical impossibility of any general motion, the result of a great transverse fracture of the strata and veins, demonstrates that no such great and general disturbance is necessary and indispensable to their production.

We must also admit that in some cases the cross-veins are, at least, as old as the ores contained in the lodes they intersect; and that in one example (2.) a movement has taken place since the materials of the cross-vein have occupied their present po-

sitions. In some instances, too, the ingredients of the cross-veins and of the adjoining rocks perfectly correspond, both in mineral composition and mechanical structure.

But the prevailing opinion, that the vein intersected must be older than that which traverses it*, if still maintained, must be received with considerable limitation†: for it not only ascribes different ages to neighbouring veins which are exactly alike in every respect, on no other evidence than that of their intersections (p, q, r), but whenever two veins mutually intersect each other at different levels (r, s, w, x, y,), this theory places its supporters in the dilemma of requiring that one is at the same time both older and newer than the other.

In the pursuit, therefore, of the severed portions of lodes which have been heaved, observation and experience are still our only guides.

In all the foregoing investigations‡ it has been assumed that any motions have been at periods subsequent to that at which the rocks became perfectly rigid and incompressible. Indeed it would have been vain to have attempted an explanation of the observed facts, on purely mechanical principles, under any other conditions.

If we suppose it possible for the rocks to have been so broken that each lode was contained in a different fragment, and that these minute masses had an independent motion, in any direction and to any extent required, although such movements might, within certain limits, have afforded any desired results, yet the motions in different directions necessary to the production of the observed phænomena, in different portions, would often have required that the rocks should in some spots have suffered much compression, whilst

* Dr. Borlase, Nat. Hist. of Cornwall (2nd edit., 1758), p. 152; Mr. Pryce, *Mineralogia Cornub.* (1778), p. 101; M. Werner, Treatise on Mineral Veins (1791), p. 53; Mr. Thomas, Report (1819), p. 21; Mr. Carne, Cornwall Geol. Trans. (1819), ii. p. 123; Mr. Hawkins, ibid. p. 232; M De la Beche, Report (1839), p. 353; Prof. Phillips, Cab. Cyclo., Geology (No. cxi. 1839), ii. p. 136.

† Dr. Boase, Primary Geology, p. 365; Mr. R. W. Fox, Report of the Royal Cornwall Polytechnic Society (1836), p. 120.

‡ The principles of simple mechanical displacement here investigated were held by M. Schmidt. They have been examined in detail by M. Zimmermann, *Die Wiederausrichtung verworfener Gänge, Lager, und Flotze* (Leipzig, 1828); more generally by myself, Proceedings of the Geological Society (1832), i. p. 406; Mr. Hopkins, Cambridge Phil. Trans. (1835) vi. p. 58; Mr. Burr, Mining Review (1836), No. viii. p. 236; Mr. R. W. Fox, Report of the Royal Cornwall Polytech. Soc. (1836), p. 120; myself, Edin. New Phil. Journal (1836), xxii. p. 161; and Reports of the British Association (1837), p. 74; Mr. De la Beche, Report (1839), p. 298; Prof. Phillips, Cab. Cyclo., Geology (No. cxi. 1839), ii. p. 145.

large vacuities must have been left in others. Had such convulsions ever taken place, traces of them must have been conspicuous; but, in fact, nothing of the kind has been detected, even in a single instance.

If, however, the movements be supposed to have taken place before the rocks had become perfectly solid, in fact, whilst they were in a state which permitted segregatory or molecular action, the question will no longer be one of simple mechanical displacement, but rather concern those forces which determine the movements of minute particles of matter.

Whether any actions thus arising could have given birth to the diversified phænomena we have considered seems to have no practical bearing, and therefore forms no part of this inquiry.

4 Clarence-street, Penzance, 1842, Sept. 29th.

LXXVIII. *Some Observations on the Electrolysis of Salts. By* Robert Hare, *M.D., Prof. of Chem. Univ. of Pennsylvania: with a Reply thereto by* J. Frederic Daniell, *For. Sec. R.S., Prof. of Chem. King's College, London; in a Letter addressed to* R. Phillips, *Esq., F.R.S., &c.*

My dear Sir,

My friend Dr. Hare has done me the favour to send me a copy of a paper which he has recently published, entitled "An Effort to refute the arguments advanced in favour of the existence, in the *Amphide Salts*, of *Radicals* consisting, like cyanogen, of more than one element." Amongst these arguments, with the majority of which I am not disposed at present to interfere, he ranks the deductions which I have drawn from my electrolytic experiments. To observations made by such high authority I am desirous of making some reply, and as the subject is of considerable importance, I am induced to think that you will endeavour to make room in the Philosophical Magazine both for so much of the paper as concerns the electrolysis, and the few brief remarks upon it which I wish to make.

Dr. Hare observes,—

"66. The last argument in favour of the existence of salt radicals, which I have to answer, is that founded on certain results of the electrolysis of saline solutions.

"67. On subjecting a solution of sulphate of soda to electrolysis, so as to be exposed to the current employed, simultaneously with some water in a voltameter, Daniell alleges that, for each equivalent of the gaseous elements of water evolved in the voltameter, there was evolved at the cathode and anode, not only a like quantity of those elements, but likewise an equal number of equivalents of soda

and sulphuric acid. This he considers as involving the necessity, agreeably to the old doctrine, of the simultaneous decomposition of two electrolytic atoms in the solution, for one in the voltameter; while, if the solution be considered as holding oxysulphionide of sodium, instead of sulphate of soda, the result may be explained consistently with the law ascertained by Faraday. In that case, oxysulphion would be carried to the anode, where, combining with hydrogen, it would cause oxygen to be extricated, while sodium, carried to the cathode, and deoxidizing water, would cause the extrication of hydrogen.

"68. Dr. Kane, alluding to the experiments above mentioned, and some others which I shall mention, alleges that '*Professor Daniell considers the binary theory of salts to be fully established by them.*'

"69. Notwithstanding the deference which I have for the distinguished inventor of the constant battery, and disinclination for the unpleasant task of striving to prove a friend to be in the wrong, being of opinion that these inferences are erroneous, I feel it to be my duty as a teacher of the science, to show that they are founded upon a misinterpretation of the facts appealed to for their justification.

"70. It appears to me, that the simultaneous appearance of the elements of water, and of acid and alkali, at the electrodes, as above stated, may be accounted for, simply by that electrolyzation of the soda, which must be the natural consequence of the exposure of the sulphate of that base in the circuit. I will, in support of the exposition which I am about to make, quote the language of Professor Daniell, in his late work, entitled 'Introduction to Chemical Philosophy,' page 413:—

"'Thus we may conceive that the force of affinity receives an impulse which enables the hydrogen of the first particle of water, which undergoes decomposition, to combine momentarily with the oxygen of the next particle in succession; the hydrogen of this again, with the oxygen of the next; and so on till the last particle of hydrogen communicates its impulse to the platinum, and escapes in its own elastic form.'

"71. The process here represented as taking place in the instance of the oxide of hydrogen, takes place, of course, in that of any other electrolyte.

"72. It is well known, that when a fixed alkaline solution is subjected to the voltaic current, the alkali, whether soda or potassa, is decomposed; so that if mercury be used for the cathode, the nascent metal, being protected by uniting therewith, an amalgam is formed. If the cathode be of platinum, the metal, being unprotected, is, by decomposing water, reconverted into an oxide as soon as evolved. This shows, that when a salt of potassa or soda is subjected to the voltaic current, it is the alkali which is the primary object of attack, the decomposition of the water being a secondary result.

"73. If in a row of the atoms of soda, extending from one electrode to the other, while forming the base of a sulphate, a series of electrolytic decompositions be induced from the cathode on the right,

to the anode on the left, by which each atom of sodium in the row will be transferred from the atom of acid with which it was previously combined, to that next upon the right, causing an atom of the metal to be liberated at the cathode; this atom, deoxidizing water, will account for the soda and hydrogen at the cathode. Meanwhile the atom of sulphate on the left, which has been deprived of its sodium, must simultaneously have yielded to the anode the oxygen by which this metal was oxidized. Of course the acid is left in the hydrous state, usually called free, though more correctly esteemed to be that of a sulphate of water.

"74. I cannot conceive how any other result could be expected from the electrolysis of the base of sulphate of soda, than that which is here described.

"75. I will, in the next place, consider the phænomena observed by Prof. Daniell, when solutions of potassa and sulphate of copper, separated by a membrane, were made the medium of a voltaic current.

"76. Of these I here quote his own account, Philosophical Magazine and Journal, vol. xvii. p. 172*.

"77. It will be admitted, that agreeably to the admirable researches of Faraday, there are two modes in which a voltaic current may be transmitted, *conduction* and *electrolyzation*. In order that it may pass by the last-mentioned process, there must be a row of anions and cathions forming a series of electrolytic atoms extending from the cathode to the anode. It is not necessary that these atoms should belong to the same fluid. A succession of atoms, whether homogeneous, or of two kinds, will answer, provided either be susceptible of electrolyzation. Both of the liquids resorted to by Daniell, contained atoms susceptible of being electrolyzed. If his idea of the composition of sulphate of copper, and the part performed by the potassa, were admitted for the purpose of illustration, we should, on one side of the membrane, have a row of atoms consisting of oxysulphion and copper; on the other, of oxygen and hydrogen.

"78. Recurring to Daniell's own description of the electrolyzing process, above quoted, an atom of copper near the anode being liberated from its anion, oxysulphion, and charged with electricity, seizes the next atom of oxysulphion, displacing and charging an atom of copper therewith united. The cupreous atom thus charged and displaced, seizes a third atom of oxysulphion, subjecting the copper, united with it, to the same treatment as it had itself previously met with. This process being repeated by a succession of similar decompositions and recompositions, an electrified atom of copper is evolved at the membrane, where there is no atom of oxysulphion. Were there *no other anion* to receive the copper, evidently the electrolyzation would not have taken place; but oxygen, on the one side of the membrane, must succeed to the office performed by oxysulphion on the other side; while hydrogen, in like manner, must succeed to the office of the copper.

"79. Such being the inevitable conditions of the process, how

* This account, having already appeared in this Journal, it is not necessary to repeat.

can it be correctly alleged by Professor Daniell, the transfer of the copper being arrested at the membrane, that as this metal '*can find nothing to combine with,*' it gives up its electrical charge to the hydrogen, which proceeds to the cathode? As hydrogen cannot be present, excepting as an ingredient in water, how can it be said that the copper can discharge itself upon the hydrogen, without combining with the oxygen necessarily liberated at the same time by the electrolytic process? How could the copper, in discharging itself to a cathion, escape a simultaneous seizure by an anion? Would not the oxidizement of this metal be a step indispensable to the propagation of that electrolytic process, by which alone the hydrogen could, as alleged, '*pass to the platinode,*' i. e. cathode?

" 80. In these strictures I am fully justified by the following allegations of Faraday, which I quote from his Researches, 826, 828:—

" 'A single ion, i. e. one not in combination with another, will have no tendency to pass to either of the electrodes, and will be perfectly indifferent to the passing current, unless it be itself a compound of more elementary ions, and so subject to actual decomposition.

" 'If, therefore, an ion pass towards one of the electrodes, another ion must also be passing simultaneously to the other electrode, although, from secondary action, it may not make its appearance.'

" 81. In explanation of the mixed precipitates produced upon the membrane, I suggest that the hydrated oxide resulted from chemical reaction between the alkali and acid, the oxide from the oxygen of the water or potassa acting as a cathion in place of that of the oxide of copper; also that the metallic copper is to be attributed to the solutions acting both as conductors and as electrolytes; so that, at the membrane, two feeble electrodes were formed, which enabled a portion of the copper to be discharged without combining with an anion, and a portion of oxygen to be discharged without uniting with a cathion. In this explanation I am supported by the author's account of a well-known experiment by Faraday, in which a solution of magnesia and water was made to act as electrodes at their surfaces respectively.

" 82. There can, I think, be no better proof that no reliance should be placed on the experiments with membranes, in this and other cases where the existence of compound radicals in acids is to be tested, than the error into which an investigator, so sagacious as my friend Professor Daniell, has been led, in explaining the complicated results.

" 83. The association of two electrolytes, and the chemical reaction between the potassa and acid, which is admitted to have evolved the hydrated oxide, seem rather to have created difficulties than to have removed them.

" 84. In this view of the subject I am supported by the opinion of Faraday, as expressed in the following language:—

" 'When other metallic solutions are used, containing, for instance, peroxides, as that of copper combined with this or any decomposable acid, still more complicated results will be obtained, which,

viewed as the direct results of electro-chemical action, will, in their proportions, present nothing but confusion; but will appear perfectly harmonious and simple, if they be considered as secondary results, and will accord in their proportions with the oxygen and hydrogen evolved from water by the action of a definite quantity of electricity.'

" 85. I cannot conceive that in any point of view the complicated and '*confused*' results of the experiment of Daniell with electrolytes separated by membranes, are rendered more intelligible by supposing the existence of salt radicals. I cannot perceive that the idea that the anion in the sulphate is oxysulphion, makes the explanation more satisfactory than if we suppose it to be oxygen. Were a solution of copper subjected to electrolysis alone, if the oxide of copper were the primary object of the current, the result would be analogous to the case of sodium, excepting that the metal evolved at the cathode, not decomposing water, would appear in metallic form. If water be the primary object of attack, the evolution of copper would be a secondary effect.

" 86. It is remarkable, that after I had written the preceding interpretation of Daniell's experiments, I met with the following deductions stated by Matteuchi as the result of an arduous series of experiments, without any reference to those of Daniell above mentioned. It will be perceived that these deductions coincide perfectly with mine.

" 87. I subjoin a literal translation of the language of Matteuchi from the *Annales de Chimie et de Physique*, tom. lxxiv. 1840, p. 110:—

" 'When salt, dissolved in water, is decomposed by the voltaic current, if the action of the current be confined to the salt, for each equivalent of water decomposed in the voltameter, there will be an equivalent of metal at the negative pole, and an equivalent of acid, plus an equivalent of oxygen, at the positive pole. The metal separated at the negative pole will be in the metallic state, or oxidized according to its nature. If oxidized, an equivalent of hydrogen will be simultaneously disengaged by the chemical decomposition of water.'

" 88. Thus it seems that the appearance of acid and oxygen at the anode, and of alkali and hydrogen at the cathode, which has been considered as requiring the simultaneous decomposition of two electrolytes upon the heretofore received theory of salts, has, by Matteuchi, been found to be a result requiring the electrolysis of the metallic base only, and consequently to be perfectly reconcilable with that theory.

" 89. In fact I had, from the study of Faraday's Researches, taken up the impression, that the separate appearance of an acid and base previously forming a salt, at the voltaic electrodes, was to be viewed as a secondary effect of the decomposition of the water or the base; so that acids and bases were never the direct objects of electrolytic transfer."

In the first place I must thank Dr. Hare for the perfect courtesy with which he has striven "to prove a friend to be in the wrong," and giving him credit for feeling, as I do, that

I had much rather be so proved to be in the wrong than that any error which I may have committed should remain uncorrected, I shall proceed to prove that, owing to his overlooking one important result of the electrolysis, he is not in the right.

It appears, then, to Dr. Hare, that in the electrolysis of saline solutions, the simultaneous appearance of the elements of water, and of acid and alkali, at the electrodes may be accounted for simply by that electrolyzation of the soda (taking sulphate of soda as an illustration) which must be the *natural consequence* of the exposure of the sulphate of that base in the circuit. I presume that by the term *natural consequence* is meant that such a result might have been, *à priori*, expected: but there is nothing, I think, that I should have anticipated less than that a solution of sulphate of soda would be affected by the voltaic current precisely in the same way as a solution of caustic soda, or that the powerful affinities of the acid and the base should have no influence upon the result.

The whole of Dr. Hare's argument is founded upon the supposition that both in an aqueous solution of soda and of sulphate of soda, "the alkali is the primary object of attack, the decomposition of the water being a secondary result." But he has neglected the important fact, that in neither case is it the sodium and the oxygen which alone *travel* in the circuit; but in the first case the water, and in the second case the acid accompanies the oxygen to the *anode*. In my experiments with the diaphragm cells, by which the liquid products of the electrolysis may be kept sufficiently separate, I have proved this over and over again. Indeed I should have joined most heartily in the wonder of my friend at the failure of my own "sagacity," if I had not established this point beyond the possibility of a doubt. It will be easy, I am sure, for Dr. Hare to convince himself by a few easy experiments that a solution of sulphate of soda submitted to electrolysis becomes acid in the zincode division of a diaphragm cell, not only by the *abstraction of sodium* from it, but by the *accumulation of acid* transferred from the platinode division; just as he will find an accumulation of soda in the latter arising from the secondary action of the sodium transferred from the former.

The same oversight of the fact, that in these processes of the electrolysis of secondary compounds the acid necessarily *accompanies* the oxygen to the zincode while the metal travels to the platinode, has misled Dr. Hare in his consideration of the phænomena observed by me when solutions of potassa and sulphate of copper, separated by a membrane, were made the medium of a voltaic circuit. It is this oversight alone which

has rendered them to him complicated and *confused*. To account for the deposition of the metal upon the diaphragm, he is reduced to the necessity of supposing that "the metallic copper is to be attributed to the solutions acting (in some manner to me unintelligible) both as conductors and electrolytes."

According to my explanation the copper is arrested on its passage to the platinode by the impossibility of its combining even temporarily with the hydrate of potassa. In its state of oxide it is also arrested on account of its being insoluble. The metal, therefore, yields its charge to the hydrogen of the hydrate which is evolved in its place at the platinode. Both the copper of the sulphate and the oxygen of the hydrate are evolved upon the membrane and enter into secondary union, if there be time for the completion of the combination; but this secondary combination is not necessary to the electrolysis, and if the process be very rapid does not completely take place.

I remain, my dear Sir, very faithfully yours,

King's College, London, J. F. Daniell.
May 11, 1843.

LXXIX. *On Æthogen and Æthonides. By* William H. Balmain*.

At the commencement of the present year I made some experiments, with the hope of obtaining compounds of boron and silicon with nitrogen; and was successful so far as to obtain compounds consisting of boron and nitrogen together with certain metals, which are possessed of some very remarkable properties. The results of these experiments, with some remarks upon their bearing upon the science of chemistry, and a few facts proving the existence of analogous compounds of silicon and nitrogen, were published in the Phil. Mag. for October, 1842. Since that time I have succeeded in isolating the compound of nitrogen and boron, and have given it the name of Æthogen, from *αἴθων* and *γείνομαι*, because it produces, by uniting with the metals, compounds which glow with a peculiarly beautiful phosphorescent light when heated before the blowpipe in the oxidizing flame. And I think its compounds may with propriety be named Æthonides.

Preparation of Æthogen.—Heat to redness seven parts of finely powdered anhydrous boracic acid with nine parts of melon, in a crucible lined with charcoal; and immediately the

* Communicated by the Chemical Society; having been read December 6, 1842.

crucible is sufficiently cool to admit of being handled, rapidly transfer the light coherent powder which will be found in the interior, to a perfectly dry well-stoppered bottle.

Properties.—A white powder, light as prepared magnesia, infusible, and fixed at a white heat. Heated before the blowpipe it burns rapidly, giving the flame a green colour, but without phosphorescing. Exposed to air for a few seconds and then heated in a tube, it yields a palpable quantity of ammonia. Heated with hydrate of potass, it yields ammonia abundantly. It is not altered by hydrogen at a low red heat, nor by chlorine at ordinary temperatures, nor by the vapour of iodine; and it is insoluble in water, but communicates to it an alkaline reaction. It is decomposed with effervescence by nitric and sulphuric acids, and there remains after evaporation boracic acid. It deflagrates with chlorate of potass and with nitre. Heated to redness with potassium and zinc it yields æthonides of those metals.

Æthonide of Potassium. Preparation.—Take seven parts of finely powdered boracic acid and twenty parts of cyanide of potassium, free from water, and, as far as possible, from cyanate of potass and iron; and having lined a Hessian crucible with a paste of powdered charcoal and gum, and heated it until all water has passed away, place the mixture in the crucible, cover it by inverting and luting a smaller crucible over it, and heat it to whiteness for an hour: it is advisable to use a crucible as a cover, that there may be sufficient room for spurious sublimation, and the rent-hole should be bored in the bottom of this crucible and not in the luting at the side; and further to avoid the penetration of oxygen to the materials, it is well to line the upper in like manner with the lower; or, by heating together potassium and æthogen avoiding an excess of potassium, and heating the result with nitric acid to free it from excess of æthogen.

Properties.—A light white solid, infusible and insoluble even when heated in water, in solution of potass, hydrochloric acid, sulphuric acid (strong and diluted), nitric acid, and solution of chlorine; it is not altered upon exposure to air, nor does it affect the most delicate turmeric paper when placed upon it in a moist state. Heated with hydrate of potass or soda, it yields ammonia abundantly. In the deoxidizing flame of the blowpipe it is not altered, nor does it communicate any colour to the flame: but in the oxidizing flame it gives a strong green colour, and gradually fuses, yielding a perfect bead which is transparent, hot and cold; and when placed with a drop of water upon test papers, turned turmeric brown and red litmus blue. When the outside flame impinges upon a large

surface of the substance in powder, as when a glass tube soiled with it is held at the extreme point of the flame, it presents a beautiful green phosphorescence, owing no doubt to the formation of boracic acid at the surface, and if it be removed to the inner flame, the centre will incandesce, while the outer edges, where it meets with the oxygen of the air, will still yield the beautiful elegant green. When thrown upon fused chlorate of potass it deflagrates with a soft green light, and it will also deflagrate with nitrate of potass. It is not altered by being gently heated with potassium or sodium, nor when heated before the blowpipe on charcoal with lead, zinc, &c. Chlorine has no action upon it at a low red heat, and iodine, sulphur and corrosive sublimate may be sublimed from it without decomposing it. It is not decomposed by hydrogen at a red heat, but below that temperature is decomposed with the evolution of ammonia by the vapour of water, or by any substance which will yield water, as hydrate of potass, hydrate of lime, common clay, hydrated phosphoric acid and the rhombic phosphate of soda. It is not decomposed by hydrochloric acid at a low red heat, and I think it is not altered by hydrofluoric acid, for a small portion of it was mixed with a large quantity of fluorspar, with more than sufficient sulphuric acid to make it all into hydrofluoric acid, and heated as long as fumes passed off, when, after the sulphate of lime had been washed away with dilute nitric acid, it still yielded ammonia with hydrate of lime.

Æthonide of Zinc. Preparation.—Heat together, to whiteness, in a lined crucible, one part of anhydrous boracic acid and two and a half parts of cyanide of zinc—or heat finely granulated zinc with æthogen to the temperature at which zinc sublimes, and wash the result with nitric acid.

Properties.—A white solid resembling the last, gives ammonia abundantly when heated with a mixture of hydrate of lime and carbonate of potass, and is insoluble (with or without heat) in water, sulphuric acid, hydrochloric acid, nitric acid, solution of potass and ammonia. It is not decomposed by chlorine or hydrogen at a *full red heat*, nor by corrosive sublimate, nor by potassium or sodium. Before the blowpipe it is infusible, but in the oxidizing flame communicates a green colour, and when at the outer edge emits a *very brilliant* bluish phosphorescence, which appearance it also produces when simply dropped into the flame of a spirit-lamp. Thrown on fused chlorate of potass, it deflagrates with a faint blue light.

Æthonide of lead may be obtained by heating chloride of lead with æthonide of zinc, or by heating boracic acid with

cyanide of lead, or by heating together lead and æthogen. It phosphoresces with a green light.

Æthonide of silver may be obtained by heating together chloride of silver and æthonide of zinc, or by heating together æthogen and silver. It is a light white solid, and is not acted upon by any of the re-agents with which it was tried, not even by chlorine or hydrogen at a full red heat. This compound phosphoresces with a peculiarly fine green light.

I believe I have obtained æthonides of several other metals by heating their chlorides with æthonide of zinc, but the quantities operated upon were too small to give certain results.

I am bound to apologize for sending in my results without an analysis, but the means of doing otherwise are not in my power, and I am in hopes that Dr. Kane will oblige us with more valuable data than I could furnish.

Liverpool, Nov. 28.

LXXX. *Report of some Experiments with Saline Manures containing Nitrogen, conducted on the Manor Farm, Havering-atte-Bower, Essex, in the occupation of Collinson Hall, Esq. By* W. M. F. CHATTERLEY*.

INDUCED by the prevailing opinion that upon the quantity of Nitrogen contained in some animal and saline manures, depend their fertilizing properties, it was decided to test, by experiment, the relative value of three saline manures containing that element as a constituent, viz. nitrate of potash, nitrate of soda, and sulphate of ammonia, all these salts, from their commercial price, being within the reach of the farmer for agricultural purposes, provided a sufficient amount of profit for the outlay can be shown, and that of course would be preferred by the agriculturist which yields the greatest profit on the prime expenditure.

For the purposes of the experiment a field of WHEAT was chosen, which in the latter end of April, 1842, presented a *thin plant*, the salts were top-dressed over the land by hand, on the 12th of May, in the quantities stated in the table below; the crop was mowed on the 10th of August, and the separate parcels taken from an eighth of an acre, threshed, measured, and weighed under my own inspection on the 24th of August. The results are as follow for the acre:—

* Communicated by the Chemical Society; having been read December 6, 1842. See Phil. Mag. S. 3. vol. xxi. p. 488.

No.	Dressing.	Cost per acre.	Produce of Corn & Straw per acre.	Produce of Corn per acre.		Weight of bu. of Corn.	Produce of Straw and Chaff per acre.		Increase of Corn per acre.		Cost of the Increase of Corn per bushel.	Increase of Straw per acre.		Total Increase per cent.	Profit on outlay per cent.
		£ s. d.	lbs.	lbs.	bu.	lbs.	lbs.	tru.	lbs.	b.p.q.	£ s. d.	lbs.	tr		
1	No Manure............	....	3700	1413	23¾	59½	2287	63½							
2	28lbs. of Sulph. Am.	0 5 10	3900	1612½	26¾	60	2287½	63½	199½	3 1 1	0 1 11½	½	—	14·1	294
3	140lbs. Ditto	1 1 9	4570	1999	32¾	61½	2571	71½	586	9 0 0	0 2 5	296	8	41·5	212
4	112lbs. Nit. Soda ...	1 4 6	4390	1905	31½	60½	2485	69	468½	7 3 1	0 3 2	198	5½	34·	138
5	112lbs. Nit. Potassa.	1 7 6	4264	1890	31¼	60½	2378	66	453½	7 2 1	0 3 8	126	2½	33·5	92

The quantity of nitrogen per cent. in each of the pure salts is as follows:—

	per cent.
Sulphate of Ammonia (crystals) .	18·80
Nitrate of Soda	16·55
Nitrate of Potassa	13·96

This calculation leaves out of the question the adventitious water contained in the salts as they are met with in commerce, and which in nitrate of soda is very considerable, sometimes as much as 10 per cent., and besides, nitrate of potassa usually containing from 2 to 12 per cent. of chloride of sodium, would in each case reduce the per centage of nitrogen. The quantity of solid impurity in the sulphate of ammonia used in these experiments was less than one per cent.

The quantity of gluten in the different samples of wheat, which forms a very considerable item in the determination of the relative value of these manures, was not obtained, in consequence of the error of the foreman, who used the reserved samples for seed; but an approximation may be obtained by comparing the weights of equal bulks, as it is constantly found that the heavier the sample, the more water is absorbed by the flour made therefrom, and consequently the more gluten it contains: by referring to the column of the table which shows the weight of a bushel of each sample, it will be seen, that for the quantities used, sulphate of ammonia (Nos. 2 and 3) is superior to the nitrates of soda or potassa (Nos. 4 and 5).

The object of these experiments having been to approximate the original cost per acre of these manures, rather than their weights, is the reason why equal weights of sulphate of ammonia and nitrates were not used: neither is it believed that the result would at all correspond with the real result of the actual experiment were we to deduct one-fifth from the

product of No. 3; indeed, the product of an acre would not then greatly exceed for 112lbs. of manure that for 26lbs.

It will be observed that the produce of the three manures does not bear a relative proportion to the quantity of nitrogen contained in each, but this gives us no reason for believing that such proportion would not hold good had the quantity of water contained in each been allowed for at the time of dressing.

The attention of the practical agriculturist will probably be attracted to the last column of this table, in which is exhibited the profit on the expenditure in each case; taking the average value of the bushel at seven shillings, and of a truss of straw at ninepence, and calculating chaff as the same value as straw for fodder. The difference between a quarter of a hundred weight and a hundred weight and a quarter of sulphate of ammonia is very remarkable, and, as may be easily perceived, is due to the difference of the value of the produce, the increase in the former instance being wholly in corn, while in the latter it is in both straw and corn, and it is probable that there is some quantity of the manure between these two extremes which would give the largest return relative to the outlay. It is clear however that if a farmer have but 1*l.* to lay out in manure, that the quantity of sulphate of ammonia to be obtained for that sum would give a larger return if spread over four acres of land than if bestowed upon one.

Sulphate of ammonia was also tried as a top-dressing upon a poor *pasture*, at the rate of one cwt. per acre; the operation was performed in the evening after a shower of rain; it was observed on the following day that the clover and some of the grass were withered, indicating that the moisture which remained on the leaves had caused the salt to adhere, and there form too strong a solution; in about a week after this period the grass assumed a greener colour, especially in one part of the field which had been dressed twice over to observe the effect: the crop of hay was not weighed, in consequence of rain coming on at the time of making, but was laid at half a load more to the acre than the undressed portion, and no doubt was entertained that by applying such a dressing earlier (it was not applied till the 12th of May), and in a less droughty season, more time would have been allowed for subsequent growth, and a much larger crop would have been obtained. The after-feed was decidedly improved, and very much preferred by the cattle.

On the same date a quantity of sulphate of ammonia at the same rate, was dressed over a single land in a field of *tares*: the leaves of the plant withered, as in the instance of the pas-

ture above-mentioned, the subsequent produce however was *one-sixth* more than any other land of the field. The oats sown with the tares grew extraordinarily after the application, and it is believed, that if applied earlier in the season to the tares as to the pasture, and during dry weather followed by less drought, the leaves of the plant would not have withered, and the effect would in both cases have been more marked.

On the 21st of May a similar quantity, viz. one cwt. to the acre, of sulphate of ammonia was applied to a field of *clover* in dry weather: in this instance the leaves did not wither, but no increased growth appeared to be the consequence; and a similar result was obtained by the application of nitrate of soda.

To a field of podding peas much infested with slug, the following dressing was applied early in the morning, and by ten o'clock an immense number of the dead insect covered the land:—

Sulphate of Ammonia ..	$7\frac{1}{2}$ cwt. at	17*s.*	0*d.*		6*l.*	7*s.*	6*d.*
Common Salt	2 cwt. at	1	6		0	3	0
Oil Cake, finely powdered	7 cwt. at	6	6		2	5	6

this was sown broadcast, at the rate of $1\frac{1}{4}$ cwt. to the acre (value 14*s.* 6*d.*), and besides saving the plant by the destruction of the insect, seemed in a short time to have converted this crop, from the worst of two others in adjoining fields, into the best of the three, and little or none of the withering effect on the leaves ensued.

It will be observed from the above experiments, that sulphate of ammonia appears to have acted better upon graminaceous than upon leguminous plants; for though the crops of tares and peas seem both to have received considerable benefit from the application, such benefit was by no means in proportion to the effect produced upon wheat or oats; the oats sown with the tares, as has been before observed, growing surprisingly after the application, and exhibiting a marked distinction in their rate of increase.

Sulphate of ammonia, as well as nitrates of soda and potassa, comes under the class of stimulating manures; that is, such manures which, at the same time that they supply the plant with one or more ingredients necessary to the crop, enable it to obtain a larger supply of its usual nourishment from the soil and the atmosphere: hence the quantity of nitrogen itself in the crop from the land to which a nitrogenous manure has been applied, is greater than the sum of the nitrogen from the unmanured portion and the quantity contained in the manure so added; and hence the result of the application of a given quantity of such manure will vary upon different soils, and even

upon the same soil under different conditions, depending upon its capacity to supply the plant with its general nourishment. On account of this stimulant action, care should be taken in the application of these manures, that the quantity used should vary according to the condition of the plant and soil, as too large a quantity on a good plant, with a soil in high condition, would cause the crop to lodge, and a small dressing can always be repeated if found necessary.

From the above experiments and several others, which want of opportunity has prevented from being carried out so fully, but from which as an eye-witness a comparative opinion may be formed, I am led to believe that no cheaper top-dressing than sulphate of ammonia can be applied to wheat or oats on this land, which is generally a heavy clay upon a subsoil of London clay, when the plant requires it, either from its being sickly or thin on the ground, in consequence of the land being somewhat out of condition, whether from unusual wet, bad seed-time, uncongenial spring, or any such-like cause. I should add that equal benefit appears to have been derived from its use upon a light gravelly soil upon a subsoil of gravel, upon the same as the London clay formation.

With respect to the quantity of the salt to be used, it may be best to refer to the practical result of a large and small dressing, as shown in the table above (Nos. 2 and 3), and the previous remarks thereon as to the relative produce of straw and corn in each case, and to add, that although the experiment has not yet been made, there seems reason to believe that a better effect would result from the application of, say one cwt. per acre at three different dressings, than all at once, that is about 37lbs. when the crop of *wheat* makes its spring growth, or when *oats* are about two inches out of the ground; a similar quantity about a month afterwards; and again at the time of the formation of the ear. But a practical difficulty occurs in applying so small a quantity as 37lbs. of any material over an acre of ground: the simplest mode of overcoming this, unless we could use a machine capable of adjustment, similar to what is termed a clover drill, perhaps is to mix the salt with some substance which shall not exert any decomposing action upon it, but so increase its bulk as to enable it to be equally spread over the surface; the best substance, as far as my experience goes, is *common salt* (itself a manure very generally useful), twice the weight of the sulphate being added to make up one cwt., a quantity not difficult to broadcast over an acre: or if preferred, *soot*, which itself contains both sulphate and carbonate of ammonia; or even such a mixture as that before-mentioned used for peas, but then care

must be taken to pulverise the oil-cake sufficiently. A method which has been found to be good is to mix *soot* and *salt* in about equal parts, some weeks previous to the addition of the sulphate of ammonia; the salt condenses moisture from the atmosphere and fixes the carbonaceous particles, and these tend to hold the carbonic acid and ammoniacal gases derived from the atmosphere and decomposing organic matters in the soil, and render them of easy access to the plants. If a very speedy effect be required, the stimulating properties of this dressing may be increased, by setting free a portion of the ammonia by a subsequent *very slight* dressing of hydrate of lime (for obvious reasons this mixture should not be made before the sowing); but as such a dressing is always attended with loss of ammonia, this should never be resorted to unless it is otherwise unavoidable, and if possible when rain may be expected to fall. And this leads me to a remark on the very common practice, in this county particularly, of mixing fermenting farm-yard manure with lime, by which all the ammoniacal salts are decomposed and the ammonia driven off, and a large portion of fertilizing material absolutely lost: it is difficult however to convince the farmer accustomed to this practice, for the immediate benefit which results is considerable; for in the first place the vegetable tissues are broken up, and put in a condition more easily to be converted into carbonic acid, and hence more easily absorbed by plants, and besides its bulk is materially diminished, so that 20 loads of the mixture contains the vegetable matter thus prepared of perhaps 40 of the manure in its ordinary state, and there can be no doubt is more speedily exhausted.

The manures here referred to, and most other saline manures when used as a top-dressing, should be applied when the plant is dry after a shower of rain, or during hazy weather, but not when any continued fall of rain is anticipated; in the first instance it is slowly dissolved by dews, &c., and permeates the soil in every part, and in the latter a considerable portion of its effect is lost as the salt is washed out and carried away in solution by the drains. When the soil is too dry, it remains inactive. It is perhaps supererogative in the present day to insist upon thorough drainage for the effectual action of any manure; it must be clear that unless the soil has been well disintegrated and a free passage exists among its particles, neither moisture, air, nor manure, can thoroughly penetrate, and this condition is effected by drainage alone.

With regard to the use of sulphate of ammonia with the drill and depositing it with the seed: in some experiments

made to determine the propriety of this mode of dressing the land, the result was a thin and bad crop, arising apparently from its having received a check during its early growth, and much of the seed having been killed as soon as germination took place; this may perhaps be accounted for by referring to the withering effect upon the leaves, observed in some of the other experiments, which may be supposed to be much more powerful upon the tender radicle and plumule in the earliest stages of development, while they are provided in the cotyledons of the seed with a mild and bland nourishment suitable for these tender organs, and before they are prepared for procuring or assimilating the stronger aliments fitted only for more mature plants.

Attention has been particularly directed to sulphate of ammonia on account of its low price as compared with other nitrogenous manures, a point upon which the extensive practical application of any manure must chiefly depend. The price paid was seventeen shillings per cwt.; it is prepared at the Gasworks in Brick Lane by a patent process for purifying coal-gas by means of dilute sulphuric acid, and is very free from impurity.

A specimen of manure sold as Daubeny's sulphate of ammonia at 12*s.* the cwt. did not give any traces of ammonia when mixed with caustic lime, but consists almost entirely of sulphate of lime, and is worth no more to the farmer than gypsum which may be obtained at 2*l.* a ton.

This manure is said to be prepared according to the directions of Dr. Daubeny of Oxford, by pouring the ammoniacal liquor of the gasworks upon finely-powdered gypsum: even if it were so, the per centage of sulphate of ammonia to be thus obtained, cannot make its value as compared with pure sulphate of ammonia as 12 to 17; and its name, "Daubeny's Sulphate of Ammonia," unqualified as it is by any explanation of its composition, is liable to lead the agriculturist unable to detect its nature, into serious loss and error.

I may perhaps be permitted to remark, that the nitrogen of coal is the store accumulated by the vegetation of past ages, before man converted it to his use, but now that this inexhaustible source of a material so necessary to increase the quantity of food to be obtained from the present race of plants is opened, it is proper to examine the most advantageous mode of employing it, that so great a boon be neither neglected nor wasted: it should therefore seem to be the duty of all who have it in their power, to confirm or refute the accuracy of such experiments as the above, and if, as I

cannot doubt, they are found to be nearly correct, to promulgate as much as possible the facts, in order that so valuable a material may be speedily appreciated by the British agriculturist.

In conclusion I have to add, that these experiments were not originally commenced with that attention to rigid accuracy which is called for in strictly scientific investigations, for they were in fact intended to serve as illustrations to a course of practical lectures on the application of science to agriculture delivered on the spot, and this may form some excuse for the omission of certain data which could easily have been obtained, but which did not appear necessary until it was decided to calculate and exhibit the results in a tabular form.

LXXXI. *On the Chemical and Contact Theories of the Voltaic Battery.* *By* Michael Faraday, *D.C.L., F.R.S., &c.**

[According to our intention expressed at page 269, we now reprint Dr. Faraday's argument against the *contact* theory of excitement in the voltaic battery, drawn from the consideration of the general and invariable laws of natural forces.—Ed.]

Improbable Nature of the Assumed Contact Force.

2065. I HAVE thus given a certain body of experimental evidence and consequent conclusions, which seem to me fitted to assist in the elucidation of the disputed point, in addition to the statements and arguments of the great men who have already advanced their results and opinions in favour of the chemical theory of excitement in the voltaic pile, and against that of contact. I will conclude by adducing a further argument founded upon the, to me, unphilosophical nature of the force to which the phænomena are, by the contact theory, referred.

2066. It is assumed by the theory (1802.) that where two dissimilar metals (or rather bodies) touch, the dissimilar particles act on each other, and induce opposite states. I do not deny this, but on the contrary think, that in many cases such an effect takes place between contiguous particles; as for instance, preparatory to action in common chemical phænomena, and also preparatory to that act of chemical combination which, in the voltaic circuit, causes the current (1738. 1743.).

2067. But the contact theory assumes that these particles, which have thus by their mutual action acquired opposite electrical states, can discharge these states one to the other, and yet remain in the state they were first in, being *in every point* entirely unchanged by what has previously taken place. It as-

* From the Phil. Trans. for 1840, p. 124.

sumes also that the particles, being by their mutual action rendered plus and minus, can, whilst under this inductive action, discharge to particles of like matter with themselves and so produce a current.

2068. This is in no respect consistent with known actions. If in relation to chemical phænomena we take two substances, as oxygen and hydrogen, we may conceive that two particles, one of each, being placed together and heat applied, they induce contrary states in their opposed surfaces, according, perhaps, to the view of Berzelius (1739.), and that these states becoming more and more exalted end at last in a mutual discharge of the forces, the particles being ultimately found combined, and unable to repeat the effect. Whilst they are under induction and before the final action comes on, they cannot spontaneously lose that state; but by removing the *cause* of the increased inductive effect, namely the heat, the effect itself can be lowered to its first condition. If the acting particles are involved in the constitution of an electrolyte, then they can produce current force (921. 924.) proportionate to the amount of chemical force consumed (868.).

2069. But the contact theory, which is obliged, according to the facts, to admit that the acting particles are not changed (1802. 2067.) (for otherwise it would be the chemical theory), is constrained to admit also, that the force which is able to make two particles assume a certain state in respect to each other, is unable to make them *retain* that state; and so it virtually denies the great principle in natural philosophy, that cause and effect are equal (2071.). If a particle of platinum by contact with a particle of zinc willingly gives of its own electricity to the zinc, because this by its presence tends to make the platinum assume a negative state, why should the particle of platinum take electricity from any other particle of platinum behind it, since that would only tend to destroy the very state which the zinc has just forced it into? Such is not the case in common induction; (and Marianini admits that the effect of contact may take place through air and measurable distances*;) for there a ball rendered negative by induction, will not take electricity from surrounding bodies, however thoroughly we may uninsulate it; and if we force electricity into it, it will, as it were, be spurned back again with a power equivalent to that of the inducing body.

2070. Or if it be supposed rather, that the zinc particle, by its inductive action, tends to make the platinum particle positive, and the latter, being in connection with the earth by other platinum particles, calls upon them for electricity, and so acquires a positive state; why should it discharge that state to

* *Memorie della Società Italiana im Modena*, 1837, xxi. 232, 233, &c.

the zinc, the very substance, which, making the platinum assume that condition, ought of course to be able to sustain it? Or again, if the zinc tends to make the platinum particle positive, why should not electricity go to the platinum *from the zinc*, which is as much in contact with it as its neighbouring platinum particles are? Or if the zinc particle in contact with the platinum tends to become positive, why does not electricity flow to it from the zinc particles behind, as well as from the platinum*? There is no sufficient probable or philosophic cause assigned for the assumed action; or reason given why one or other of the consequent effects above mentioned should not take place: and, as I have again and again said, I do not know of a single fact, or case of contact current, on which, in the absence of such probable cause, the theory can rest.

2071. The contact theory assumes, in fact, that a force which is able to overcome powerful resistance, as for instance that of the conductors, good or bad, through which the current passes, and that again of the electrolytic action where bodies are decomposed by it, can arise out of nothing; that, without any change in the acting matter or the consumption of any generating force, a current can be produced which shall go on for ever against a constant resistance, or only be stopped, as in the voltaic trough, by the ruins which its exertion has heaped up in its own course. This would indeed be *a creation of power*, and is like no other force in nature. We have many processes by which the form of the power may be so changed that an apparent *conversion* of one into another takes place. So we can change chemical force into the electric current, or the current into chemical force. The beautiful experiments of Seebeck and Peltier show the convertibility of heat and electricity; and others by Œrsted and myself show the convertibility of electricity and magnetism. But in no cases, not even those of the Gymnotus and Torpedo (1790.), is there a pure creation of force; a production of power without a corresponding exhaustion of something to supply it†.

* I have spoken, for simplicity of expression, as if one metal were active and the other passive in bringing about these induced states, and not, as the theory implies, as if each were mutually subject to the other. But this makes no difference in the force of the argument; whilst an endeavour to state fully the joint changes on both sides, would rather have obscured the objections which arise, and which yet are equally strong in either view.

† (*Note*, March 29, 1840).—I regret that I was not before aware of most important evidence for this philosophical argument, consisting of the opinion of Dr. Roget, given in his Treatise on Galvanism in the Library of Useful Knowledge, the date of which is January 1829. Dr. Roget is, upon the facts of the science, a supporter of the chemical theory of excitation;

2072. It should ever be remembered that the chemical theory sets out with a power, the existence of which is pre-proved, and then follows its variations, rarely assuming anything which is not supported by some corresponding simple chemical fact. The contact theory sets out with an assumption, to which it adds others as the cases require, until at last the contact force, instead of being the firm unchangeable thing at first supposed by Volta, is as variable as chemical force itself.

2073. Were it otherwise than it is, and were the contact theory true, then, as it appears to me, the equality of cause and effect must be denied (2069.). Then would the perpetual motion also be true; and it would not be at all difficult, upon the first given case of an electric current by contact alone, to produce an electro-magnetic arrangement, which, as to its principle, would go on producing mechanical effects for ever.

Royal Institution, Dec. 26, 1839.

LXXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 157.]

December 8, 1842.—The following papers were read, viz.:—

"Observations on the Blood-corpuscles, particularly with reference to opinions expressed and conclusions drawn in papers 'On the Corpuscles of the Blood,' and 'On Fibre,' recently published in the Philosophical Transactions." By T. Wharton Jones, Esq., F.R.S.

The author points out what he considers to be important errors in the series of papers by Dr. Martin Barry, which have lately appeared in the Philosophical Transactions, and are entitled, "*On the Corpuscles of the Blood*," and "*On Fibre**." He alleges that Dr. Barry has

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but the striking passage I desire now to refer to, is the following, at § 113. of the article Galvanism. Speaking of the voltaic theory of contact, he says, "Were any further reasoning necessary to overthrow it, a forcible argument might be drawn from the following consideration. If there could exist a power having the property ascribed to it by the hypothesis, namely, that of giving continual impulse to a fluid in one constant direction, without being exhausted by its own action, it would differ essentially from all the other known powers in nature. All the powers and sources of motion, with the operation of which we are acquainted, when producing their peculiar effects, are expended in the same proportion as those effects are produced; and hence arises the impossibility of obtaining by their agency a perpetual effect; or, in other words, a perpetual motion. But the electromotive force ascribed by Volta to the metals when in contact, is a force which, as long as a free course is allowed to the electricity it sets in motion, is never expended, and continues to be excited with undiminished power, in the production of a never-ceasing effect. Against the truth of such a supposition, the probabilities are all but infinite."—Roget.

* See Phil. Mag., S. 3. vol. xx. pp. 321, 344; vol. xxi. p. 220.—Edit.

generally confounded the colourless corpuscles contained in the blood with the red corpuscles of the same fluid ; each of which latter kind consists of a vesicle or cell, with thick walls, but in a collapsed and flattened state, and having therefore a biconcave form, and in consequence of its thick wall being doubled on itself, presenting under the microscope a broad circumferential ring, which is illuminated or shaded differently from the depressed central portion, according to the focal adjustment of the instrument: while the colourless corpuscles, on the other hand, are of a globular shape, strongly refractive of light, and granulated on their surface, and are of less specific gravity and of somewhat larger size than the red corpuscles. The author quotes various passages from Dr. Barry's papers in proof of his assertions, and refers particularly to fig. 23 of his second paper on the corpuscles of the blood. He farther states, that Dr. Barry's description of the appearances of what he terms the red corpuscles, in paragraphs 53, 68, and 76 of his second paper, can, in fact, apply only to the colourless corpuscles: and he observes, that even when Dr. Barry does, at last, in his "Additional Observations," advert to the distinction between the red and the colourless globules, he considers the latter as being merely "the discs" contained in the red globules appearing under an altered state.

The author regards as wholly erroneous the notion which Dr. Barry entertains that a fibre exists in the interior of the blood-corpuscle; and that these fibres, after their escape from thence, constitute the fibres which are formed by the consolidation of the fibrin of the *liquor sanguinis*. The beaded aspect presented by the double contour of the thick wall of the red corpuscle when it has been acted upon either by mechanical causes or by chemical reagents, of which the effect is to corrugate the edge, and to bend it alternately in opposite directions, has, in the opinion of the author, given rise to the illusive appearance of an internal, annular fibre. The appearance of flask-like vesicles presented by some of the red corpuscles, with the alleged fibre protruding from their neck, the author ascribes altogether to the effects of decomposition, which has altered the mechanical properties of the corpuscle, and allowed it to be drawn out, like any other viscid matter, into a thread.

In conclusion, he remarks, that if these statements of Dr. Barry should be recognised as fundamental errors in his premises, the whole of the reasonings built upon them must fall to the ground.

2. "Wind Table, from observations taken at the summit of the Rock of Gibraltar." By Colonel George J. Harding. Communicated by Captain Beaufort, R.N., F.R.S., by order of the Lords Commissioners of the Admiralty.

3. "Spermatozoa observed within the Mammiferous Ovum." By Martin Barry, M.D., F.R.S. L. and Ed.

In examining some ova of a rabbit, of twenty-four hours, the author observed a number of spermatozoa in their interior.

Dec. 15.—A paper was read, entitled "Experimental Inquiry into the cause of the Ascent and Continued Motion of the Sap; with a new method of preparing plants for physiological investigations."

By George Rainey, Esq., M.R.C.S. Communicated by P. M. Roget, M.D., F.R.S.

The ascent of the sap in vegetables has been generally ascribed to a vital contraction either of the vessels or of the cells of the plant: the circumstances of that ascent taking place chiefly at certain seasons of the year, and of the quantity of fluid, and the velocity of its motion being proportional to the development of those parts whose functions are obviously vital, as the leaves and flowers, have been regarded as conclusive against the truth of all theories which professed to explain the phenomenon on purely mechanical principles. The aim of the author, in the present paper, is to show that these objections are not valid, and to prove, by a series of experiments, that the motion of the sap is totally independent of any vital contractions of the passages which transmit it; that it is wholly a mechanical process, resulting entirely from the operation of endosmose; and that it takes place even through those parts of a plant which have been totally deprived of their vitality.

The lower extremity of a branch of *Valeriana rubra* was placed, soon after being gathered, into a solution of bichloride of mercury. In a few hours a considerable quantity of this solution was absorbed, and the whole plant, which had been previously somewhat shrunk from the evaporation of its moisture, recovered its healthy appearance. On the next day, although the lower portion of the branch had lost its vitality, the leaves and all the parts of the plant into which no bichloride had entered, but only the water of the solution, were perfectly healthy and filled with sap. On each of the following days additional portions of the stem became affected in succession; but the unaffected parts still preserved their healthy appearance, and the flowers and leaves developed themselves as if the plant had vegetated in pure water and the whole stem had been in its natural healthy state. On a minute examination it was found that calomel, in the form of a white substance, had been deposited on the internal surface of the cuticle; but no bichloride of mercury could be detected in those parts which had retained their vitality; thus showing that the solution of the bichloride had been decomposed into chlorine, calomel, and water, and had destroyed the vitality of the parts where this action had taken place; after which, fresh portions of the solution had passed through the substance of the poisoned parts, as if they had been inorganic canals. Various experiments of a similar kind were made on other plants, and the same conclusions were deduced from them.

As the addition of a solution of iodide of potassium converts the bichloride of mercury into an insoluble biniodide, the author was enabled, by the application of this test to thin sections of the stems of plants into which the bichloride had been received by absorption, to ascertain, with the aid of the microscope, the particular portion of the structure into which the latter had penetrated. The result of his observations was, that the biniodide is found only in the intercellular and intervascular spaces, none appearing to be contained within the cavities of either cells or vessels.

As the fluids contained in the vessels and in the cells hold in solution various vegetable compounds, their density is greater than the ascending sap, which is external to them, and from which they are separated by an intervening organized membrane. Such being the conditions requisite for the operation of the principle of endosmose, the author infers that such a principle is constantly in action in living plants; and that it is the cause of the continual transmission of fluids from the intervascular and intercellular spaces into the interior of the vessels and cells, and also of the ascent of the sap.

Dec. 22.—A paper was in part read, entitled "On the Nerves:" by James Stark, M.D., F.R.S.E. Communicated by James F. W. Johnston, Esq., F.R.S., Professor of Chemistry in the University of Durham.

Jan. 12, 1843.—1. The reading of a paper, entitled "On the Nerves," by James Stark, M.D., was resumed and concluded.

The author gives the results of his examinations, both microscopical and chemical, of the structure and composition of the nerves; and concludes that they consist, in their whole extent, of a congeries of membranous tubes, cylindrical in their form, placed parallel to one another, and united into fasciculi of various sizes; but that neither these fasciculi nor the individual tubes are enveloped by any filamentous tissue; that these tubular membranes are composed of extremely minute filaments, placed in a strictly longitudinal direction, in exact parallelism with each other, and consisting of granules of the same kind as those which form the basis of all the solid structures of the body; and that the matter which fills the tubes is of an oily nature, differing in no essential respect from butter, or soft fat; and remaining of a fluid consistence during the life of the animal, or while it retains its natural temperature, but becoming granular or solid when the animal dies, or its temperature is much reduced. As oily substances are well known to be non-conductors of electricity, and as the nerves have been shown by the experiments of Bischoff to be among the worst possible conductors of this agent, the author contends that the nervous agency can be neither electricity, nor galvanism, nor any property related to those powers; and conceives that the phenomena are best explained on the hypothesis of undulations or vibrations propagated along the course of the tubes which compose the nerves, by the medium of the oily globules they contain. He traces the operation of the various causes which produce sensation, in giving rise to these undulations; and extends the same explanation to the phenomena of voluntary motion, as consisting in undulations, commencing in the brain, as determined by the will, and propagated to the muscles. He corroborates his views by ascribing the effects of cold in diminishing or destroying both sensibility and the power of voluntary motion, particularly as exemplified in the hybernation of animals, to its mechanical operation of diminishing the fluidity, or producing solidity, in the oily medium by which these powers are exercised.

2. A letter from Prof. Hansen to G. B. Airy, Esq., F.R.S., A.R.,

was also read, "On a New Method of computing the Perturbations of the Planets whose eccentricities and inclinations are not small." Communicated by G. B. Airy, Esq., F.R.S.

The author announces that he has found a method by which the absolute perturbations of planets for any given time, with any given eccentricity and inclination of the orbit, may be calculated; and he exemplifies his method by applying it to the computation of the perturbations produced by Saturn on the comet of Encke, in every point of its orbit; a problem of which hitherto there existed no solution*.

3. A paper was also in part read, entitled "On the minute structure of the Skeletons or hard parts of the Invertebrata." By W. B. Carpenter, M.D. Communicated by the President.

Jan. 19.—The following papers were read:—

1. "Variation de la Déclinaison et Intensité Horizontale observées à Milan pendant vingt-quatre heures consécutives le 25 et 26 Novembre, et le 21 et 22 Décembre 1842." Par Prof. Carlini, For. Mem. R.S.

2. The reading of a paper, entitled "On the minute structure of the Skeletons or hard parts of Invertebrata," by W. B. Carpenter, M.D., was resumed and concluded.

The present memoir is the first of a series which the author intends to communicate to the Society, and relates only to the Mollusca; and he proposes, hereafter, to extend his inquiries to the skeletons of the Echinodermata, and the various classes of articulated animals. After adverting to the classifications of shells proposed by Mr. Hatchett and Mr. Gray †, from the propriety of which he finds reason to dissent, he proceeds to state the results of his microscopic examination of the texture of shells under the several following heads. First, shells having a prismatic cellular structure, as the Pinna, and which are composed of a multitude of flattened hexagonal calcareous prisms, originally deposited in continuous layers of hexagonal cells, and thus constituting a calcified epithelium, analogous with the enamel of the teeth. Secondly, those consisting of membranous shell-substance, the basis of which, after the removal of its calcareous portion, presents nothing but a membranous film, of greater or less consistence, composed of several layers, but without the appearance of any cellular tissue: this membrane the author regards as being derived from the mantle, of which it was originally a constituent part, by the development of nucleolated cells; and the various corrugations and foldings of which it is susceptible in different species, introducing many diversities into the structure of the shells of this class. Thirdly, shells having a nacreous structure, and exhibiting the phenomena of iridescence; a property which the author ascribes to the plicated form of the membrane of the shell, combined with a secondary series of transverse corrugations. Fourthly, shells exhibiting a tubular structure, formed by cylindrical perforations occurring among the several layers, and

* See pres. vol. p. 303.—Edit.

† See Phil. Mag., S. 3. vol. iii. p. 452.—Edit.

varying in diameter from about the 20,000th to the 3500th part of an inch; but measuring on an average about the 6000th part of an inch, and presenting a striking analogy with the dentine or ivory of the teeth. The last sections of the paper relate to the epidermis and the colouring matter of shells.

References are made, in many parts of the paper, to illustrative drawings; which, however, the author has not yet supplied.

Jan. 26.—The following papers were read, viz.—

1. "Observations on certain cases of Elliptic Polarization of Light by Reflection," by the Rev. Baden Powell, M.A., F.R.S., Savilian Professor of Geometry in the University of Oxford.

The author, by way of introduction, passes in review the labours of various inquirers on the subject of the elliptic polarization of light, and notices more particularly those of Sir David Brewster, who first discovered this curious property, as recorded in the Philosophical Transactions for 1830; of Mr. Airy, in the Cambridge Transactions for 1831 and 1832; and of Professor Lloyd, in the Philosophical Transactions for 1840, and in the Reports of the British Association for 1841. He then proceeds to give an account of his own experimental examination of the phenomena of elliptic polarization in the reflection of light from various surfaces, by observing the modifications of the polarized rings under different conditions, both of surface and of incidence, and by endeavouring to ascertain both the existence and amount of ellipticity, as shown by the dislocation of those rings, and to determine its peculiar character, as indicated by the direction in which the dislocation takes place; the protrusion of the alternate quadrants appearing, in certain cases, in one direction, and in others in the opposite. These observations are reducible to two classes; first, those designed to contribute to the inquiry, what substances possess the property of elliptic polarization, by examining the light reflected from various bodies; and second, those made on certain cases of films of several kinds, including those formed on metals by oxidation or other action upon the metal itself, as well as by extraneous deposition. The author found the general result, in all these cases, to be, that from any one tint to another, through each entire order of tints, the form of the rings in the reflected light undergoes certain regular changes; passing from a dislocation in one direction to that in the opposite, through an intermediate point of no dislocation, or of plane polarization; and thus exhibiting a dark and a bright centred system alternately, as long as the order of tints is preserved pure. These changes in the form of the rings, he observes, are precisely those expressed by successive modifications of Mr. Airy's formula, corresponding to the increments in the retardation which belong to the periodical colours of the films.

The remaining portion of the paper is occupied by a description of the apparatus and mode of conducting the experiments; and of the observations made on mica, on decomposed glass, plumbago, daguerreotype, and other metallic plates, and on the coloured films produced on steel and on copper by the action of heat, and of voltaic

electricity. The author gives, in conclusion, an analytical investigation of Mr. Airy's general formula.

2. "Variation of the Magnetic Needle as observed at Washington City, D. C., from $3^h\ 30^m$ July 24th to 3^h July 25th, 1840, inclusive (Göttingen mean time)," by Lieut. Gillies, of the United States Service. Communicated by Samuel Hunter Christie, Esq., Sec. R.S.

Feb. 2.—A paper was read, entitled "Experimental Researches in Electricity:" Eighteenth Series; by Michael Faraday, Esq., D.C.L., F.R.S. Section 25. On the Electricity evolved by the Friction of Water and Steam against other bodies.

The object of the experiments related in this paper, is to trace the source of the electricity which accompanies the issue of steam of high pressure from the vessels in which it is contained. By means of a suitable apparatus, which the author describes and delineates, he found that electricity is never excited by the passage of pure steam, and is manifested only when water is at the same time present; and hence he concludes that it is altogether the effect of the friction of globules of water against the sides of the opening, or against the substances opposed to its passage, as the water is rapidly moved onwards by the current of steam. Accordingly it was found to be increased in quantity by increasing the pressure and impelling force of the steam. The immediate effect of this friction was, in all cases, to render the steam or water positive, and the solids, of whatever nature they might be, negative. In certain circumstances, however, as when a wire is placed in the current of steam at some distance from the orifice whence it has issued, the solid exhibits the positive electricity already acquired by the steam, and of which it is then merely the recipient and the conductor. In like manner, the results may be greatly modified by the shape, the nature, and the temperature of the passages through which the steam is forced. Heat, by preventing the condensation of the steam into water, likewise prevents the evolution of electricity, which again speedily appears by cooling the passages so as to restore the water which is necessary for the production of that effect. The phænomenon of the evolution of electricity in these circumstances is dependent also on the quality of the fluid in motion, more especially in relation to its conducting power. Water will not excite electricity unless it be pure; the addition to it of any soluble salt or acid, even in minute quantity, is sufficient to destroy this property. The addition of oil of turpentine, on the other hand, occasions the development of electricity of an opposite kind to that which is excited by water; and this the author explains by the particles or minute globules of the water having each received a coating of oil in the form of a thin film, so that the friction takes place only between that external film and the solids, along the surface of which the globules are carried. A similar, but a more permanent effect is produced by the presence of olive oil, which is not, like oil of turpentine, subject to rapid dissipation.

Similar results were obtained when a stream of compressed air

was substituted for steam in these experiments. When moisture was present, the solid exhibited negative, and the stream of air positive electricity; but when the air was perfectly dry, no electricity of any kind was apparent. The author concludes with an account of some experiments in which dry powders of various kinds were placed in the current of air; the results differed according to the nature of the substances employed, and other circumstances*.

Feb. 9.—The following papers were read, viz.—

1. "Magnetical Term-Observations made at the Observatory at Prague, for September, October, November and December, 1842:" by Professor Kreil. Communicated by S. Hunter Christie, Esq. Sec. R.S.

2. "On the Structure and Mode of Action of the Iris:" by C. R. Hall, Esq. Communicated by P. M. Roget, M.D., Sec. R.S.

After reciting the various discordant opinions entertained at different periods by anatomists and physiologists, relative to the structure and actions of the iris, the author proceeds to give an account of his microscopical examination of the texture of this part of the eye, in different animals. He considers the radiated plicæ, which are seen on the uvea in Mammalia, as not being muscular; but he agrees with Dr. Jacob in regarding them as being analogous in structure to the ciliary processes. The white lines and elevations apparent on the anterior surface of the human iris, he supposes to be formed by the ciliary nerves which interlace with one another in the form of a plexus. The iris, he states, is composed of two portions; the first, consisting of a highly vascular tissue, connected by vessels with the choroid, ciliary processes, sclerotica and cornea, and abundantly supplied with nerves, which, in the human iris, appear, in a front view, as thread-like striæ; and which are invested, on both surfaces, by the membrane of the aqueous humour. They are more or less thickly covered with pigment, which, by its varying colour, imparts to the iris on the anterior surface its characteristic hue; and, by its darkness on the posterior surface, renders an otherwise semi-transparent structure perfectly opake. The second component portion of the iris consists of a layer of concentric muscular fibres, which fibres, in Man and Mammalia generally, are situated on the posterior surface of the pupillary portion of the iris; but which in Birds extend much nearer to the ciliary margin, and consequently form a much broader layer. In Fishes and in some Reptiles they do not exist at all.

The author then proceeds to inquire into the bearings which these conclusions may have on the physiology of the iris. He thinks that the phenomena of its motions can receive no satisfactory explanation on the hypothesis of erectility alone, or on that of the antagonism of two sets of muscular fibres; the one for dilating, the other for contracting the pupil. He is convinced that the contraction of the pupil is the effect of muscular action; but does not consider the knowledge we at present possess is sufficient to enable us to determine the nature of the agent by which its dilatation is effected.

* On the subject of Dr. Faraday's paper see the articles referred to at p. 5 of the present volume.—Edit.

He, however, throws it out as a conjecture, that this latter action may be the result of an unusual degree of vital contractility, residing either in the cellular tissue, or in the minute blood-vessels of the iris. It is from elasticity, he believes, that the iris derives its power of accommodation to changes of size, and its tendency to return to its natural state from extremes, either of dilatation or of contraction; but beyond this, elasticity is not concerned in its movements.

Feb. 16.—The following papers were read, viz.—

1. "Tide-Observations at Tahiti:" by Captain Edward Belcher, R.N. Communicated by Captain Beaufort, R.N., F.R.S., &c.

This paper consists of copies of the Tide Journal, registered at the Island of Motuatu, in the Harbour of Papeete, and of a short comparative series made at Point Venus. They were conducted by Mr. McKinley Richardson, Mate. The construction of the tide-gauge is described; and an account is given of the methods of observation, and of the precautions adopted to ensure accuracy. The results are specified in the following letter from the author to Captain Beaufort, which accompanies the paper:—

"Her Majesty's Ship Sulphur, Woolwich, August 2, 1842.

"Sir,—Referring to the Tide Registries, forwarded on my arrival, I beg leave to offer the following general remarks upon the tides at Tahiti.

"In consequence of your very special instructions relative to the determination of the *actual periods* of high water at the Island of Tahiti, the most minute attention was paid to this subject; and as these periods could only be *approximated*, recourse was had to my old method (successfully practised in the Lancashire survey), of deriving them from the Equal-altitude system.

"By a reference to the Tide Registry annexed, it will be found that there are *two distinct periods of high water*, during each interval of twenty-four hours; and that during the seven days preceding, and seven days following the full and change, they are confined between the limits of 10 A.M. and 2^h 30^m P.M., the whole range of interval, by day as well as by night, being about 4^h 27^m.

"Commencing with the seventh day preceding the full moon, viz. the 9th of April, it will be perceived that high water occurs at 10 A.M., this being the greatest A.M. interval from noon; and that on the 16th, at the full moon, it occurs nearly at noon.

"Passing on to the 23rd, it reaches the greatest P.M. limit at 2^h 30^m, and on the 2nd of May again reaches the noon period.

"Between the 23rd and 24th, however, a sudden anomaly presents itself. Throughout the day of the 23rd, the variation of the level does not exceed $2\frac{1}{2}$ inches, and the general motion is observed to be 'irregular.' The time of high water is also the extreme P.M. limit.

"On the 24th we discover that it has suddenly resumed *the most distant* A.M. *period*, viz. 10 A.M., but proceeds regularly to the noon period at the change.

"Although the differences of level do not at full and change exceed 1 foot $4\frac{1}{2}$ inches, still I presume that we have sufficient data to establish the fact,—that it is *not invariably high water at noon*

(as asserted by Kotzebue, Beechey and others); and, further, that we have corresponding *nightly periods* of high water.

"It is evident that the time of high water at full and change may be assumed as that of noon, because we have sufficiently decided changes of level to fix the approximate period of high water.

"It does not appear by these Registers, that any higher levels result from the rollers sent in by the strong sea breezes (as asserted by several writers), but rather the contrary, the highest levels being indicated during the night, when the land breezes prevailed.

"I have great satisfaction in presenting you with these facts, and trust that they may induce others to follow up the same experiments, so as, eventually, to obtain the variations which other seasons may produce.

"I am, Sir, your most obedient servant,

"EDWARD BELCHER, *Captain.*"

"*Captain Beaufort, R.N., F.R.S., Hydrographer.*"

2. "On Fissiparous Generation:" by Martin Barry, M.D., F.R.S. L. and Ed.

The author observes that the blood-corpuscle and the germinal vesicle resemble one another* in the circumstance of an orifice existing in the centre of the parietal nucleus of both. He pursues the analogy still farther, conceiving that as a substance of some sort is introduced into the ovum through its orifice, which the author terms *the point of fecundation*, so the corpuscles of the blood may undergo a sort of fecundation through their corresponding orifice; and also that the blood-corpuscle, like the germinal vesicle, is propagated by self-division of its nucleus; a mode of propagation which he believes to be common to cells in general. The nucleus of the germinal vesicle, or original parent cell of the ovum, gives origin, by self-division, to two young persistent cells, endowed with qualities resulting from the fecundation of the parent cell; these two cells being formed by assimilation, out of a great number of minuter cells which had been previously formed. This account of the process, which takes place in the reproduction of the entire organism, explains, according to Dr. Barry, the mysterious reappearance of the qualities of both parents in the offspring.

Certain nuclei, which the author has delineated in former papers as being contained within and among the fibres of the tissues, he conceives to be, in like manner, centres of assimilation, from observing that they present the same sort of orifice, that they are reproduced by self-division, and that they are derived from the original cells of development; that is, from the nuclei of the corpuscles of the blood. He considers that assimilation of the substance introduced into the parietal nucleus of the cell is part of the process which propagates the cell; that the mode of reproduction of cells is essentially fissiparous, and that the process of assimilation prepares them for being cleft.

A pellucid point is described by the author as being "contained

* Dr. Barry requests us to add, that the words "in certain states" are wanted here.—EDIT.

in a certain part of the cell-wall, and as representing the situation of a highly pellucid substance, originally having little if any colour." This substance, which he considers as being primogenital and formative, he denominates *hyaline*, and ascribes to it the following properties. It appropriates to itself new matter, thus becoming enlarged; then divides and subdivides into globules, each of which passes through changes of the same kind. Under certain circumstances, it exhibits a contractile power, and performs the motions called *molecular*. It is the seat of fecundation, and it is by its successive divisions that properties descend from cell to cell, new properties being continually acquired as new influences are applied; but the original constitution of the hyaline not being lost. The main purpose for which cells are formed is to reproduce the hyaline; and this they do by effecting the assimilation which prepares it to divide; such division being thus the essential part of fissiparous generation.

The remaining part of the paper is occupied with a detailed account of these processes as they occur in the development of the ovum, and also in the changes exhibited by the corpuscles of the blood, in which fissiparous reproduction also takes place, and the red blood-discs are converted into fibrin, and thus give origin to the various tissues of the organs. The same theory of fissiparous reproduction he also applies to the formation of the muscular fibre, in connexion with his belief that it is composed of a double spiral filament. Contractile cilia, he supposes, are also formed by the elongation of nuclei, the filaments proceeding from them in opposite directions. The author considers, lastly, the subject of the fissiparous reproduction of the Infusoria, and particularly of the *Volvox globator*, the *Chlamido-monas*, *Baccillaria*, *Gonium*, and the *Monadina* in general; and applies the same theory to gemmiparous reproduction, and to the so-called spontaneous generation of infusoria and parasitic entozoa.

LONDON ELECTRICAL SOCIETY.

[Continued from p. 232, and from vol. xxi. p. 485.]

Dec. 20, 1842.—"Memoir on the Difference between Leyden Discharges and Lightning-Flashes, and on their relative action upon Metallic Bodies vicinal to the conductor of the respective discharges." By Charles V. Walker, Esq. Hon. Sec.

The author commences by quoting largely from the writings of various electricians, to show that the *habit* of using Leyden discharges in *many* lightning experiments has led to their adoption in *all*; and then points out three circumstances in which great *degrees* of difference exist; the comparative *distance* of the clouds operating in producing a great *excess* of electricity in them, over and above what is expelled into the earth by induction; the *greater area* of the earthy disc operating to produce in it a very low *degree of tension*; and the *inferior conductibility* of the earth causing a *maximum of resistance* to the diffusion of the flash towards restoration of equilibrium.

These positions were supported by several new experiments with the double electroscope. The author then explains an imperfect state of charge consequent on the discharge of a system which developes on the outer coating a certain quantity of free electricity, to which Mr. Walker gives the name "induced residual." He assumes a case of 100 Harris's units being thrown on one coating of a jar while 90 are expelled from the other, on the well-known principle that one coating always possesses an *excess* over the other; the outer coating in this case has a capacity and attraction for 90. If, however, the discharge is made and 90 pass, the whole of the 90 cannot abide there, because the 10 remaining in the jar would require a dispersion of 9 from the outer coating. This 9 is termed the "induced residual," and is shown to bear direct ratio to the excess and the flash. If n represent the ratio of excess and ϕ the flash, the induced residual x is equal to $\left(1-\frac{1}{n}\right).\phi$. But in the case of clouds both n and ϕ are very great, and therefore the free electricity x is at a maximum. The author then shows experimental illustrations of lateral sparks, and traces their dependence on this induced residual; from which he draws the inference of similar phænomena occurring on an immensely larger scale in the discharge of lightning, and illustrates his inference by the phænomena observed by Mr. Weekes with his atmospheric apparatus, and also by experiments with the prime conductor, which he shows to be not so dissimilar to a charged cloud as some have imagined. He now traces the action of the part of the flash actually required to compensate the excited area of the earth; its value is $\frac{1}{n}.\phi$; it enters the earth at *one* spot, and is required for compensating a *large area*. The comparatively low conducting power of the earth greatly resists its diffusion, and induces a tendency to divide into several paths; and possibly some portion even of this may be converted into free electricity, from the fact that the compensation of the extreme verge of the disc could be more readily effected by the return of the electricity from the adjacent lower stratum than by the diffusion of the flash from the one centre. Mr. Walker then reports several instances of accidents having occurred from the division of lightning-flashes from the conducting rod over other adjacent bodies, even when the rod was of *standard size*, and *perfect* in all its parts. From all which he infers that it is a matter of vital importance to connect such bodies with the rod, an inference which he confirms by a mass of testimony from the best sources. In the course of the memoir a modification of the disc experiment is introduced, in which the vicinal body is *between* the discs, but connected with the earth by means of a wire passing through a glass tube in the lower disc. Sparks *occur* in this case as they did in the experiment of erecting rods on the floor of the room. Mr. Walker then shows how a *copper-bottomed* vessel protected by Mr. Harris's conductor resembles the lower disc, and, from the fact of no spark passing when the vicinal metal touches the lower disc, he infers, *à fortiori*, that no spark can leave one of Mr. Harris's rods

to a metal body *not* connected with the sheathing, and therefore that the system adopted by that gentleman is the best which human prudence could suggest. The memoir contains numerous extracts from all authentic sources in illustration of the several positions very briefly noticed in this report.

ROYAL IRISH ACADEMY.

[Prof. MacCullagh's Note continued from p. 413.]

II.—*On Fresnel's Formula for the Intensity of Reflected Light, with Remarks on Metallic Reflexion.*

When Mr. Potter discovered, by experiment, that more light is reflected by a metal at a perpendicular incidence than at any oblique incidence (at least as far as 70°), the fact was looked upon, by himself and others, as contrary to all received theories; and certainly the universal opinion, up to that time, was, that the intensity of reflexion always increases with the incidence. It may therefore be worth while to remark, that the formula given by Fresnel for reflexion at the surface of a transparent body, though not of course applicable, except in a very rude way, to the case of metals, would yet lead us to expect, for highly refracting bodies as the metals are supposed to be, precisely such a result as that obtained by Mr. Potter. For when the index of refraction exceeds the number $2 + \sqrt{3}$, or the tangent of 75°, the expression for the intensity of reflected light will be found to have a *minimum* value at a certain angle of incidence; while for all less values of the refractive index the intensity will be least at the perpendicular incidence.

Let i and i' be the angles of incidence and refraction, and put

$$\mathrm{M} = \frac{\sin i}{\sin i'}, \quad \mu = \frac{\cos i}{\cos i'};$$

then if I be the intensity of the reflected light, when common light is incident, Fresnel's expression

$$\mathrm{I} = \tfrac{1}{2}\left\{\frac{\sin^2(i-i')}{\sin^2(i+i')} + \frac{\tan^2(i-i')}{\tan^2(i+i')}\right\},$$

in which the intensity of the incident light is taken for unity, may be put under the form

$$\mathrm{I} = \frac{\left(\frac{1}{\mu} - \mu\right)^2 + \left(\frac{1}{\mathrm{M}} - \mathrm{M}\right)^2}{\left(\frac{1}{\mu} + \mu + \frac{1}{\mathrm{M}} + \mathrm{M}\right)^2},$$

which has a minimum value when

$$\mu + \frac{1}{\mu} = \mathrm{M} + \frac{1}{\mathrm{M}} - \frac{8}{\mathrm{M} + \frac{1}{\mathrm{M}}};$$

the value of I being in that case

$$\mathrm{I} = \frac{\left(\mathrm{M} - \frac{1}{\mathrm{M}}\right)^2 - 4}{\left(\mathrm{M} - \frac{1}{\mathrm{M}}\right)^2} = \frac{\left(\mathrm{M} + \frac{1}{\mathrm{M}}\right)^2 - 8}{2\left(\mathrm{M} + \frac{1}{\mathrm{M}}\right)^2 - 8},$$

and the corresponding angle of incidence being given by the formula

$$\sin i = \frac{\sqrt{M}\,\sqrt[4]{\varepsilon^2-1}}{\varepsilon+\sqrt{\varepsilon^2-1}}, \text{ where } \varepsilon = \tfrac{1}{4}\left(M+\frac{1}{M}\right).$$

Since $\mu + \dfrac{1}{\mu}$ cannot be less than 2, it is easy to see that, when there is a minimum, $M+\dfrac{1}{M}$ cannot be less than 4, and therefore M cannot be less than $2+\sqrt{3}$, or 3·732.

As an example, let $M+\dfrac{1}{M}=6$. Then, at a perpendicular incidence, one-half the incident light will be reflected. The minimum will be when $i = 65° 36'$, and at this angle only $\frac{7}{16}$ of the incident light will be reflected. The value here assumed for the refractive index is that which Sir J. Herschel (Treatise on Light, Art. 594) assigns to mercury; but if my ideas be correct, it is far too low for that metal.

The only person who supposes that the refractive index of a metal is not a large number, is M. Cauchy. It has always been held as a maxim in optics, that the higher the reflective power of any substance, the higher also is its refractive index. But M. Cauchy completely reverses this maxim; for, as I have elsewhere shown (*Comptes Rendus*, tom. viii. p. 964), it follows from his theory that the most reflected metals are the least refractive, and even that the index of refraction, which for transparent bodies is always greater than unity, may for metals descend far below unity. Thus, according to his formula, the index of refraction for pure silver is the fraction $\frac{1}{4}$, so that the dense body of the silver actually plays the part of a very rare medium with respect to a vacuum. It appears to me that such a result as this is quite sufficient to overturn the theory from which it is derived. The formulas, however, which he gives for the intensity of the reflected light, are identical with the empirical expressions which I had given long before, and are at least approximately true.

In framing my own empirical theory (see Proceedings, vol. i. p. 2), two suppositions relative to the value of the refractive index presented themselves. Putting M for the *modulus*, and χ for the *characteristic*, I had to choose between the values $M\cos\chi$ and $\dfrac{M}{\cos\chi}$. The latter value is that which I adopted; the former, which is M. Cauchy's, was rejected because I saw that it would lead to the result above mentioned.

Another result of M. Cauchy's, which he has given twice in the *Comptes Rendus* (tom. ii. p. 428, and tom. viii. p. 965), requires to be noticed. When a polarized ray is reflected by a metal, the phase of its vibration is altered, and if the incidence be oblique, the change of phase is different, according as the light is polarized in the plane of incidence, or in the perpendicular plane. But when the ray is reflected at a perpendicular incidence, it is manifest that the change is a constant quantity, whatever be the plane of polarization.

In fact, the distinction between the plane of incidence and the perpendicular plane no longer exists, and the phænomena must be the same in all planes passing through the ray. Yet M. Cauchy, in the two places above quoted, asserts it to be a consequence of his theory, that in this case the alterations of phase are different for two planes of polarization at right angles to each other, and that the difference of the alterations amounts to half an undulation. The same singular hypothesis had been previously made by M. Neumann (Poggendorff's *Annals*, vol. xxvi. p. 90), whom M. Cauchy appears to have followed: but M. Neumann has since admitted it to be erroneous (*Ibid.* vol. xl. p. 513).

The Chair having been taken, *pro tempore*, by the Rev. J. H. Todd, D.D., V.P., the President (Sir W. R. Hamilton) communicated the following proof of the known law of Composition of Forces.

Two rectangular forces, x and y, being supposed to be equivalent to a single resultant force p, inclined at an angle v to the force x, it is required to determine the law of the dependence of this angle on the ratio of the two component forces x and y.

Denoting by p' any other single force, intermediate between x and y, and inclined to x at an angle v', which we shall suppose to be greater than v; and denoting by x' and y' the rectangular components of this new force p', in the directions of x and y, we may, by easy decompositions and recompositions, obtain a new pair of rectangular forces, x'' and y'', which are together equivalent to p', and have for components

$$x'' = \frac{x}{p} x' + \frac{y}{p} y';$$

$$y'' = \frac{x}{p} y' - \frac{y}{p} x';$$

the direction of x'' coinciding with that of p', but the direction of y'' being perpendicular thereto. Hence

$$\frac{y''}{x''} = \frac{x y' - y x'}{x x' + y y'};$$

that is, $$\tan^{-1}\frac{y''}{x''} = \tan^{-1}\frac{y'}{x'} - \tan^{-1}\frac{y}{x};$$

or, finally, $$f(v'-v) = f(v') - f(v), \quad \ldots\ldots \quad \text{(A.)}$$

at least for values of v, v', and $v'-v$, which are each greater than 0, and less than $\frac{\pi}{2}$; if f be a function so chosen that the equation

$$\frac{y}{x} = \tan f(v)$$

expresses the sought law of connexion between the ratio $\frac{y}{x}$ and the angle v. The functional equation (A.) gives

$$f(mv) = mf(v) = \frac{m}{n} f(nv),$$

m and n being any whole numbers; and the case of equal compo-

nents gives evidently

$$f\left(\frac{\pi}{4}\right)=\frac{\pi}{4};$$

hence

$$f\left(\frac{m}{n}\,\frac{\pi}{4}\right)=\frac{m}{n}\,\frac{\pi}{4},$$

and ultimately,

$$f(v)=v, \quad \ldots\ldots\ldots\ldots \quad (B.)$$

because it is evident, by the nature of the question, that while v increases from 0 to $\frac{\pi}{2}$, the function $f(v)$ increases therewith, and therefore could not be equal thereto for all values of v commensurable with $\frac{\pi}{4}$, unless it had the same property also for all intermediate incommensurable values. We find, therefore, that for all values of the component forces x and y, the equation

$$\frac{y}{x}=\tan v \quad \ldots\ldots\ldots\ldots \quad (C.)$$

holds good; that is, the resultant force coincides *in direction* with the diagonal of the rectangle constructed with lines representing x and y as sides.

The other part of the known law of the composition of forces, namely, that this resultant is represented also *in magnitude* by the same diagonal, may easily be proved by the process of the *Mécanique Céleste*, which, in the present notation, corresponds to making

$$x'=x, \quad y'=y, \quad x''=p,$$

and therefore gives

$$p=\frac{x^2+y^2}{p}, \; p^2=x^2+y^2.$$

But the demonstration above assigned for the law of the *direction* of the resultant, appears to Sir William Hamilton to be new.

LXXXIII. *Intelligence and Miscellaneous Articles.*

ON THE THEORY OF GLACIERS, WITH REFERENCE TO A FORMER COMMUNICATION. BY J. SUTHERLAND, M.D.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the last Number of the Philosophical Magazine there is a short abstract of a paper on the theory of glaciers read by me before the Liverpool Literary and Philosophical Society on the 14th of November 1842, and which was forwarded to you at my request by the Secretary. The object of the paper was to prove that the discoveries of M. Agassiz, in regard to the infiltration of water into glacier ice, might be made the basis of a theory which would afford an explanation of all the known facts in regard to glacier motion. I stated the chief of these facts, and amongst others mentioned Professor Forbes's discovery of a motion in glaciers similar to that of fluids,

and explicitly attributed it to him. I find on reperusing the abstract, that though such is the case, this discovery is also included along with the other facts in the general theory proposed, and that from its being apparently a necessary consequence of the theory the merit of its originality might appear lessened. Such however is not the case: what the theory does is rather to confirm its truth, of which Professor Forbes has brought abundance of evidence from other sources.

If you will give this Note a place in your forthcoming Number you will oblige,

Gentlemen, yours, &c.,

Liverpool, March 10, 1843. JOHN SUTHERLAND.

ON THE COMBUSTION OF IRON PYRITES FOR THE MANUFACTURE OF SULPHURIC ACID.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

In the combustion of iron pyrites for the purposes of sulphuric acid manufacture, it is well known that sulphate of iron is more or less formed; the proportion will depend on the quality of the pyrites, and the degree of perfectness with which it is burnt. On examination, I find that the sulphate of iron formed is the sesquisulphate, whose symbol is $Fe_2O_3 + 3SO_3$. A careful analysis of the average run of pyrites ashes gives 3·20 per cent. as sulphuric acid.

It was my intention to have sent you a more detailed account of my experiments on this subject, had not other demands on my time obliged me to leave them imperfect; should returning leisure enable me to finish them, if acceptable they shall be at your service.

I remain, Gentlemen, your obedient Servant,

April 23, 1843. A SUBSCRIBER.

OXIDE AND PEROXIDE OF BISMUTH—BISMUTHIC ACID.

M. Frémy states that when hydrated oxide of bismuth is boiled in a solution of alkali, a period arrives in which the precipitate, which was originally white, is converted into a considerable quantity of small yellow brilliant needles, which are anhydrous oxide of bismuth.

He also states, that by acting on oxide of bismuth by the alkalies at a high temperature, he has been able to obtain peroxide of bismuth of perfect purity. This oxide has been mentioned by several chemists, but has not hitherto been obtained in a state of purity. M. Jacquelain found that when oxide of bismuth is heated with an alkali in a silver crucible, it is peroxidized and combines with the alkali; but he could not obtain the peroxide in a separate state. M. Frémy obtained in the same way bismuthate of soda, but he found that if this salt is boiled with an excess of soda, the acid is dehydrated and quits the alkali. The peroxide thus obtained is of a puce colour, like the peroxide of lead, and may be washed with nitric acid without being decomposed. By analysis this oxide was found to be

represented by Bi^2O^4. Thus the two oxides of bismuth, says M. Frémy, have evidently for their formulæ Bi^2O^3, Bi^2O^4. This result agrees perfectly with the experiments of M. Jacquelain, and the late experiments of M. Regnault on the specific heat of bismuth, and that of its combinations.—*Journal de Pharmacie*, Janvier 1843.

ON THE OXIDES OF LEAD, PLOMBIC ACID AND THE PLOMBATES.

M. Frémy observes, that the protoxide of lead dissolves in the alkalies, and forms crystalline compounds with some bases; but the hydrate of this oxide, under the influence of the alkalies, is dehydrated as readily as the hydrates of the oxides of bismuth and tin; so that when the hydrated protoxide of lead is boiled in a solution of alkali not sufficient to dissolve it, the hydrate is converted into perfectly crystallized anhydrous oxide of lead; it is the oxide which M. Payen had previously obtained by treating acetate of lead with ammonia. This oxide, he has also remarked, may change its colour when strongly heated, and the same effect may be produced by friction.

The solutions of oxide of lead in the alkalies deposit by evaporation anhydrous crystals of oxide of lead, which differ from the preceding by the facility with which they dissolve even in weak alkaline solutions.

M. Frémy states, that the observations which he has made respecting the protoxide of lead, prove that it may combine with bases when hydrated, but that, like the protoxide of tin, it is dehydrated under the influence even of the alkalies which hold it in solution, and that it then precipitates in the anhydrous crystalline state, possessing different properties according to the different circumstances which determine its precipitation.

The puce or peroxide of lead has hitherto been considered as a neutral body, incapable of combining with any other substance, and all chemists have considered minium as a compound of protoxide and peroxide of lead. According to M. Frémy, the peroxide of lead is a true acid, which is capable of combining with bases to form well-defined and often crystallized salts, the general formula of which is PbO^2, MO. He proposes then to give the second compound of lead with oxygen the name of *plombic acid*, then reserving the name of *plombites* for the salts formed by the combination of protoxide of lead with metallic oxides.

The plombates are prepared in the dry way: the plombates of potash and soda are obtained by heating peroxide of lead (plombic acid) with excess of these alkalies; the mass is to be treated with water; the liquor by spontaneous evaporation yields perfectly defined crystals of the alkaline plombate. These salts may also be procured by heating in the air protoxide of lead with the alkali, which becomes peroxide and oxidizes the protoxide of lead.

The plombates of potash and soda crystallize perfectly in a weak alkaline solution, but are decomposed by pure water; consequently when a solution of a plombate is diluted with a large quantity of

water, it soon becomes of a deep red colour, and deposits plombic acid; acids, added to the plombates, occasion the precipitation of plombic acid.

All plombates are obtained by calcining in the air a mixture of metallic lead and protoxide of lead. Thus, then, minium or red lead is one of the series of plombates; it is a plombate of protoxide of lead. It is well known that when a metal forms both an oxide and an acid, they generally exist in combination; as examples of this may be cited the chromate of chromium, tungstate of tungstenum, stannate of tin, &c. &c. Minium is then also arranged in this series of compounds.—*Ibid.*

ON AMMONIA-AMIDIDE OF HYDROGEN. BY PROF. DANIELL.

Professor Daniell observes, that " if we add to a cold solution of bichloride of mercury a very slight excess of ammonia, a copious white precipitate is formed, and the liquid is found to contain exactly half the chlorine of the bichloride combined with hydrogen and ammonia as muriate of ammonia. The white powder, which has long been known by the name of *white precipitate of mercury*, contains all the mercury and the remaining half of the chlorine. Dr. Kane believes that it is a compound of chloride and *amidide* of mercury, and that its formula, adopting 202, the ordinary number for mercury, is $Hg\ Cl_2 + 2\ N\ H_2$.

" An amidide of mercury has, however, never been obtained in a separate state.

" When potassium is heated in dry ammoniacal gas, hydrogen is set free, and a compound is formed, which is a fusible solid of an olive-green colour, which has been supposed to be an amidide of potassium, or $Ka,\ N\ H_2$, but it likewise contains undecomposed ammonia. It has, however, been observed, that if ammonia were simply reduced to the state of amidogen in this process, 4 volumes should be decomposed and evolve 2 volumes of hydrogen, but in the numerous experiments of Gay-Lussac and Thenard, never more than $3\frac{1}{2}$ volumes were required to furnish 2 volumes of hydrogen, so that the constitution of the green substance must be considered as very problematical.

" Such is the evidence upon which we are required to review all the compounds into which ammonia enters with reference to this new radical, which has never been isolated or transferred, and to consider ammonia itself as an *amidide of hydrogen*, or $N\ H_2 + H$.

"Ammonium, which we have considered as the radicle of the common salts of ammonia (an hypothesis which we have found to be so remarkably confirmed by the results of electrolysis), is then a subamidide of hydrogen, or $N\ H_2 + H_2$; and sulphate of ammonia $N\ H_2 + H_2 + O + S\ O_3$, or a sulphate of the subamidide of hydrogen; and oxalate of ammonia, $N\ H_2 + H_2 + O + C_2\ O_3$, an oxalate of the oxide of subamidide of hydrogen, and so on with the salts of the other acids.

" An immense amount of ingenuity has been expended upon this hypothesis, but, as the nature of chemical analysis has been most

happily illustrated by the resolution of a word into its letters*, so we cannot help being reminded by this and similar transpositions of elements, of that ingenious exercise of the mind which is afforded by the literary conceits called *anagrams*; in which the letters of a word are required to be transposed so as to form another word; unfortunately, however, the true chemical combination is not, in general, so obvious as the literal.

"The hypothesis of amidogen does not appear to clear up any of the difficulties which attach to some of the ammoniacal compounds, and is therefore objectionable, as unnecessarily introducing a confusion of ideas and nomenclature which is much to be deprecated in elementary teaching."—*Daniell's Chem. Phil.*, edit. ii. p. 671.

LARGE MASS OF NATIVE GOLD FOUND IN THE OURAL MOUNTAINS.

Humboldt lately transmitted to the Academy of Sciences of Paris, a notice by M. de Koscharoff, an officer of the Russian Mines, regarding a mass of gold of large size, recently found in the Oural. The largest mass of native gold which had previously been found in the Oural Mountains, weighed upwards of 22 pounds avoirdupois; and it is that of which there is a plaster model in the Museum of Natural History at Paris. On the 7th of November last, however, there was found in the same mountains a mass of native gold, weighing about 80 pounds avoirdupois.

The mines of Zarevo-Nicolaefsy and of Zarevo-Alexandrofsy, situated in the alluvial auriferous deposits of Miass, on the Asiatic side of the southern portion of the Oural, have already afforded more than 13,300 avoirdupois pounds of gold. It was in this alluvium that, in 1836, the large mass of 22 pounds, and several others of from 8 to 14 pounds, were found at the depth of a few inches under the surface.

Subsequently to the year 1837, the mines of Nicolaefsy and Alexandrofsy seeming exhausted, new explorations were made in the neighbourhood, and especially along the river Tashnow-Targanna. Great success attended the search for gold in the marshy plain, and the whole valley had been searched except that part of it occupied by the building in which the washing operations were carried on.

In 1842 it was resolved to remove the houses, whereupon sands were met with of immense richness, and lastly there was discovered, under the corner of a building, at a depth of three yards, a mass of gold weighing more than 79 pounds avoirdupois. This mass is placed in the collection of the *Corps des Mines*, at St. Petersburgh. According to the information given by M. de Humboldt in the third volume of his *Examen Critique de la Géographie du nouveau Continent*, the mass of gold found in the Oural, in 1826, was inferior in weight to that discovered in 1502 in the alluvium of the island of Haïti, and inferior also to that found in 1821 in the United States.

* "Whewell, Philosophy of the Inductive Sciences, vol. i. p. 362."

in the county of Cavarras, and described by M. Zoehler, a pupil of the Freyberg School of Mines. The mass found at Miass, fifteen years ago, weighs $22\frac{1}{4}$ pounds; that of Cavarras, $27\frac{3}{4}$ pounds; that of Haïti, from 30 to 34; but the mass of gold found in November 1842, in beds of alluvium reposing on diorite, is more than twice the weight of the largest of these, as it weighs nearly 80 pounds. Such has been the prodigious increase of the quantity of gold obtained by washing in Russia, and especially in Siberia, to the east of the southern chain of the Oural, that, according to very accurate data, the total produce during the year 1842 amounted to upwards of 35,000 pounds avoirdupois.—*L'Institut*, No. 472.

FAHLERZ CONTAINING MERCURY, FROM HUNGARY.

Professor Zeuschner procured this remarkable Fahlerz during his geognostical tour in Hungary, and wished it to be analysed, on account of its containing mercury. It occurs at Kotterbach in the vicinity of Iglo, and is very probably the same compact fahlerz containing mercury, from Poratsch in Upper Hungary, which Klaproth analysed. The ore is only found in a massive state, and is frequently interspersed with copper pyrites, from which the portions destined for analysis were carefully purified. Hr. Scheidthauer performed the following three analyses of the ore in the laboratory of Professor H. Rose, but it was only in one of them that all the component parts were determined. In the second analysis, from particular causes, the whole amount of mercury could not be obtained; and in the third the sulphur alone was determined:—

	I.	II.	III.
Sand of grains of quartz...	2·73	1·82	1·87
Antimony..............	18·48	18·50	...
Arsenic................	3·98	4·10	...
Iron	4·90	5·05	...
Zinc	1·01	1·02	...
Copper	35·90	35·87	...
Mercury	7·52	...	...
Sulphur	23·34	23·70	23·90
Silver and lead	traces.		
	97·86		

Poggendorff's *Annalen*, 1843. No. 1. p. 161. Jameson's Journ.

ON ARSENIO-SIDERITE, A VARIETY OF ARSENIATE OF IRON. BY M. DUFRENOY.

M. Lacroix, pharmacien of Mâcon, sent to me some months since, specimens of a fibrous substance, of a yellowish brown colour, found in the manganese mine of Romanèche near Mâcon.

The fibrous arrangement of this substance, connected with its locality, led to the supposition that it was hydrated peroxide of manganese, its colour bearing some resemblance to the specimens from Romanèche.

The analysis which I have made of this substance has not con-

firmed the suspicion, but has proved that it contains arsenic acid, peroxide of iron and lime, and that it is a double arseniate, differing in its characters and composition from the previously known arseniates.

The proportions of its constituents are as follows:—

Arsenic acid	34·26
Oxide of iron	41·31
Oxide of manganese	1·29
Lime	8·43
Silica	4·04
Potash	0·76
Water	8·75
	98·84

which may be represented by the formula

$$3\ F^{2}\ Ar + C\ Ar^{2} + 3\ Aq \ldots + S.$$

In this formula, I have considered the gelatinous silica as foreign to the mineral. The analysis of the limestone of Champigny, near Paris, which contains 10 of every 100 of silica soluble in acids, without the smallest admixture of alumina, that of the green grit of Vouziers, given by M. Sauvage in his important work on the Geology of the Ardennes, which shows that this rock contains 56 of every 100 of silica soluble in a solution of potash, prove with certainty that gelatinous silica is mechanically mixed with minerals, the definite proportions of which clearly show that they contain no combined silica. Silica frequently occurs in solution in the same waters that deposit carbonate of lime; it appears that the same has occurred with the mineral from Romanèche, which has all the characters of a concretion, intermixed with gelatinous silica.

Arsenic and iron being the two elements of this new substance, I have given it the name of arsenio-siderite, which designates them.

The arsenio-siderite forms concretionary fibrous masses, which adhere to the surfaces of the tubercles of manganese; these fibres, which are large and distinct, may be separated like those of hard asbestus: it is tender and readily yields to the fingers. Its colour is yellowish-brown, which deepens by exposure to the air. It is very fusible by the blowpipe, and exhibits the reactions of both arsenic and iron. Its specific gravity is 3·52.—*Ann. de Chim.* Mars 1843.

ANALYSIS OF CHRYSOBERYL. BY M. AWDEJEW.

The author of these analyses remarks, that few minerals have afforded more variable results than the chrysoberyl. Klaproth and Arfwedson considered it to be a silicate of alumina; Seybert first proved it to contain glucina; he considered the chrysoberyl as containing silicate of alumina, and aluminate of glucina. Thomson stated that it contained no alumina, which has been confirmed by the analysis of M. H. Rose.

M. Awdejew examined two varieties of chrysoberyl, one from Brazil, and the other from Ural; the latter has been described by M. G. Rose.

Chrysoberyl from Brazil.—This mineral was in the state of yellow

transparent crystals of the size of a nut; its density was 3·733. It was pulverized in a steel mortar and fused with bisulphate of potash. The free mass was completely dissolved in water, which proves the absence of silica. The solution contained traces of alumina, and was precipitated by excess of ammonia; the oxides thus precipitated were dissolved in hydrochloric acid, and the solution was decomposed in the cold by a solution of potash; the oxide of iron precipitated was again dissolved in hydrochloric acid, and then precipitated by ammonia; the glucina was separated by Gmelin's process, by boiling the solution much diluted; from the solution afterwards acidulated by hydrochloric acid, the alumina was precipitated by ammonia. The mean of two analyses gave

Glucina	18·0
Alumina	78·4
Protoxide of iron	3·9
	100·3

Chrysoberyl from the Ural.—This mineral contained traces of the oxides of copper and lead, which were separated by passing sulphuretted hydrogen into the solution obtained after the action of the bisulphate of potash; in other respects the analysis was conducted as above, except that the glucina being treated with a mixture of carbonate and nitrate of potash, a little oxide of chromium was separated from it; the chromic acid was reduced to the state of protoxide of chromium by means of hydrochloric acid, and then precipitated by ammonia. 100 parts yielded—

Glucina	18·02
Alumina	78·92
Protoxide of iron	3·12
Oxide of chromium	0·36
Oxide of copper and lead	0·29
	100·71

Regarding this mineral as an aluminate of glucina, it would be composed of

Alumina	80·25
Glucina	19·75
	100·

Ann. de Chim. et de Phys., Fevrier 1843.

SOLUTION OF AN EQUATION. BY JAMES COCKLE, B.A., TRINITY COLLEGE, CAMBRIDGE.

To the Editors of the Philosophical Magazine.

Gentlemen,

I have taken the liberty of sending to you a solution of the equation

$$x^3 + a x + b = 0 \quad \ldots\ldots \quad (1.)$$

Assume $$x^3 + 3 p x^2 + 3 p^2 x = - b \quad \ldots\ldots \quad (2.)$$

Subtract (1.) from (2.) and divide by $3 p x$: then

$$x + p = \frac{a}{3 p} \quad \ldots\ldots \quad (3.)$$

Add p^3 to each side of (2.): then

$$(x + p)^3 = p^3 - b = \left(\frac{a}{3p}\right) \text{ by (3.),}$$

$$\text{or} \quad p^6 - b\,p^3 = \left(\frac{a}{3}\right)^3$$

a quadratic in p^3 $\therefore$ p is known, and by (3.)

$$x = -p + \frac{a}{3p}.$$

Yours very respectfully,
James Cockle,
(*B.A. Trin. Coll. Cam.*).

Middle Temple, Feb. 10, 1843.

ON THE VARIATION OF GRAVITY IN SHIPS' CARGOES.

(From another Correspondent.)

A communication appeared in the last Number of this Journal "On the effect of the variation of gravity on ships' cargoes in different latitudes," the author of which appears to have forgotten that *two* scales are commonly used in weighing goods, and that the weights in *both* must be equally affected by "variation of gravity."

A ton of any kind of goods weighed at the King's beam in London, and shipped for Madras, will on arriving there counterpoise a standard ton weight as it did before shipment, unless an addition or subtraction of weight has taken place during the voyage. K. K.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1843.

Chiswick.—April 1. Rain. 2. Cloudy : clear and fine. 3. Slight rain : clear. 4. Rain : cloudy and windy. 5. Fine. 6. Overcast : slight rain. 7. Rain : clear and fine. 8. Clear and fine. 9. Easterly haze. 10. Clear and fine. 11. Frosty : clear and dry : frosty at night. 12. Sharp frost : clear : cloudy : clear and frosty at night. 13. Cloudy : frosty at night. 14. Uniformly overcast. 15. Hazy. 16. Hazy : fine. 17, 18. Light haze : dry air : clear and fine. 19. Dry easterly haze. 20, 21. Very fine. 22. Showery. 23, 24. Cloudy and fine. 25. Rain : clear. 26. Cold rain : very fine : rain at night. 27. Cloudy and fine. 28. Slight rain. 29, 30. Cloudy and fine.—Mean temperature of the month $0^\circ \cdot 36$ above the average.

Boston.—April 1. Rain : rain early A.M. 2. Rain : rain early A.M. : rain P.M. 3. Cloudy. 4. Cloudy : rain early A.M. : stormy with rain P.M. 5. Windy. 6. Cloudy : rain A.M. 7. Cloudy. 8. Fine : rain P.M. 9. Cloudy. 10. Fine : snow A.M. : snow P.M. 11—13. Fine. 14. Windy : rain early A.M. 15, 16. Cloudy. 17—21. Fine. 22. Cloudy : rain early A.M. 23. Fine : rain early A.M. 24. Fine. 25. Cloudy. 26. Fine: rain P.M. 27. Fine : rain A.M. 28. Windy. 29. Fine : rain A.M. 30. Cloudy.

Sandwick Manse, Orkney.—April 1. Damp : fog. 2. Bright : damp. 3. Bright : fog. 4. Cloudy. 5. Damp : drizzle. 6. Rain : clear : aurora. 7. Cloudy : rain. 8. Cloudy : snow-showers. 9. Snow-showers : hail-showers. 10. Snow-drift : showers. 11. Snow-showers. 12. Snowing : drift. 13. Snow-showers : snowing. 14. Bright : cloudy. 15. Cloudy. 16. Clear. 17. Clear : cloudy. 18. Cloudy : damp. 19. Cloudy. 20. Damp : cloudy. 21. Damp : fog. 22. Damp : rain. 23. Damp. 24. Bright : cloudy. 25, 26. Rain. 27. Bright : cloudy. 28. Rain. 29. Fog : cloudy. 30. Fog : bright : fine.

Applegarth Manse, Dumfries-shire.—April 1, 2. Heavy rain. 3. Fair and fine. 4. Rain. 5. Fair and fine : aurora. 6, 7. Rain. 8. Rain : thunder : hail. 9. Fair : frost. 10, 11. Hail : frost. 12. Frost A.M. : snow : rain. 13. Frost. 14—16. Fair and temperate. 17. Fair and temperate : fine spring day. 18—20. Fair and temperate. 21. Fair and temperate : showers. 22. Rain nearly all day. 23. Rain A.M. 24. Rain early . very fine day. 25. Heavy rain : flood. 26. Fair. 27. Fair : hoar-frost. 28. Heavy rain. 29. Fair and fine. 30. Fair.

Meteorological Observations made at the Apartments of the Royal Society, LONDON, *by the Assistant Secretary*, Mr. Roberton; *by* Mr. Thompson, *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *by the* Rev. W. Dunbar, *at Applegarth Manse*, DUMFRIES-SHIRE; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month.	Barometer.								Thermometer.										Wind.					Rain.					Dew-point.
1843. April.	London: Roy. Soc. 9 a.m.	Chiswick: Max.	Chiswick: Min.	Boston. 8½ a.m.	Dumfries-shire. 9 a.m.	Dumfries-shire. 9 p.m.	Orkney, Sandwick. 9½ a.m.	Orkney, Sandwick. 8½ p.m.	London: R.S. Fahr. 9 a.m.	London: R.S. Self-reg. Mx.	London: R.S. Self-reg. Mn.	Chiswick. Max.	Chiswick. Min.	Boston. 8½ a.m.	Dumf.-shire. Max.	Dumf.-shire. Min.	Orkney, Sandwick. 9¾ a.m.	Orkney, Sandwick. 8½ p.m.	London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond. R.S. 9 a.m.
1.	29·540	29·500	29·484	29·03	29·17	29·08	29·27	29·16	51·3	58·0	49·6	59	50	51	52	43	42	43½	s.	sw.	calm	sw.	e.	·102	·18	·31	......	·10	48
2.	29·510	29·649	29·452	28·98	29·20	29·25	29·21	29·32	54·7	59·0	51·3	59	48	51	53	43	48	45	s.	sw.	calm	s.		·141	·05	·11	......	·07	50
3.	29·840	29·798	29·572	29·22	29·50	29·50	29·48	29·60	56·3	60·2	50·8	61	46	51·5	56	45	48	43	sw var	sw.	w.	w.		·052	·22	·08	......	......	50
4.	29·396	29·553	29·305	28·96	29·21	29·20	29·52	29·52	51·3	61·3	49·0	57	41	49	56	42	47	43	s.	s.	calm	s.	se.	·263	·23	·10	......	......	50
5.	29·764	29·917	29·702	29·17	29·30	29·70	29·61	29·74	49·8	56·3	43·7	56	38	48	54	41½	41½	41½	sw.	w.	w.	w.	se.	·283	·02	·66	......	·02	47
6.	29·870	29·843	29·616	29·36	29·45	29·30	29·47	29·33	51·3	57·2	45·0	55	51	47	53	36½	41½	40½	s.	sw.	w.	s.	s.	·058	·01	......	......	·17	47
☽ 7.	29·586	29·563	29·436	28·95	29·23	29·12	29·15	29·12	53·6	56·8	50·2	62	44	52·5	52	41	48	41½	s.	sw.	calm	sw.	s.	......	·35	·05	1·37	......	52
8.	29·612	29·573	29·544	28·90	29·09	29·50	29·36	29·68	51·0	61·7	47·6	59	39	51	54	38	42	36½	s.	w.	w.	nnw.	nne.	·450	·01	......	......	·41	48
9.	29·636	29·703	29·598	29·25	29·62	29·70	29·74	29·77	44·9	58·6	44·8	47	32	43	47	35	36	34	ne.	e.	calm	nne.	n.	......	......	·12	......	·06	47
10.	29·938	29·921	29·892	29·44	29·78	29·81	29·78	29·80	41·7	48·3	36·4	50	26	40	45	29	36	35	nw.	n.	calm	n.	n.	......	·01	......	......	·16	40
11.	30·010	29·983	29·971	29·53	29·88	29·88	29·94	29·93	39·5	48·0	35·2	49	22	35·5	44½	26½	32½	31	wnw.	n.	nw.	nw.	nw.	·055	......	·11	......	·20	32
12.	29·970	29·949	29·829	29·59	29·85	29·61	29·75	29·82	39·3	45·7	33·0	48	28	35	46	26	38	33	nw.	ne.	calm	nw.	ne.	......	·04	......	......	·24	35
13.	29·830	29·983	29·797	29·43	29·82	29·78	29·94	29·56	38·3	45·0	35·0	48	24	39	41½	28	33	33	nw.	n.	n.	n.	nw.	·065	......	......	......	·14	34
○ 14.	29·914	30·006	29·914	29·40	29·52	29·73	29·44	29·74	44·3	46·0	34·0	54	42	42·5	55	33½	46	45	w.	ne.	w.	w.	w.	......	......	·10	......	·41	35
15.	30·138	30·108	30·091	29·61	29·89	29·85	29·80	29·83	51·0	53·3	44·0	58	47	51	55	44½	49	46	w.	sw.	calm	sw.	sw.	......	......	......	0·38	·17	43
16.	29·968	29·992	29·818	29·53	29·80	29·81	29·89	29·91	54·0	57·6	49·2	63	42	51	56	43	48	42½	e.	e.	calm	s.	ssw.	......	......	......	......	·04	45
17.	29·964	30·048	29·854	29·40	29·86	29·90	30·03	30·08	52·0	60·3	45·7	65	33	52	60	42	45	42½	n.	ne.	calm	s.	nw.	......	......	......	......	......	48
18.	30·150	30·140	29·980	29·60	29·90	29·88	29·99	29·87	52·7	61·0	45·8	67	35	52	55	38	48½	46½	ne.	e.	calm	s.	se.	......	......	......	......	......	42
19.	29·992	29·959	29·766	29·44	29·81	29·71	29·86	29·89	45·7	62·2	41·8	63	41	53	63½	45	50	43	e.	e.	calm	sse.	n.	......	......	......	......	......	45
20.	29·736	29·778	29·690	29·23	29·60	29·61	29·82	29·73	57·3	60·4	45·8	70	36	54	59	43	43½	41	s.	sw.	calm	e & sw	n.	......	......	......	......	......	49
☾ 21.	29·902	29·849	29·833	29·27	29·67	29·66	29·73	29·76	55·7	67·3	49·3	65	45	58·5	58½	44	47	41½	w.	sw.	calm	sw.	se.	......	·05	......	......	·19	51
22.	29·876	29·989	29·815	29·25	29·59	29·70	29·80	29·94	54·3	65·6	51·0	59	26	51·5	53½	45½	45	43	ssw.	sw.	calm	sw.	ese.	·050	·03	·06	......	......	52
23.	30·104	30·043	30·025	29·56	29·78	29·85	29·99	29·98	50·3	63·7	42·3	60	28	48	50½	40	43	43	s.	s.	calm	sw.	ese.	·041	......	·03	1·19	·25	39
24.	30·060	30·012	29·994	29·50	29·90	29·79	30·00	29·76	50·7	58·6	42·3	60	27	51	58	37	45	43	e.	e.	calm	ssw.	se.	......	......	......	......	......	45
25.	29·774	30·070	29·648	29·28	29·29	29·38	29·27	29·38	47·7	57·0	42·0	56	34	45	52	40½	42	43	sse.	s.	sw.	w.	se.	......	·30	......	......	·29	44
26.	29·670	29·677	29·626	29·20	29·39	29·51	29·32	29·50	41·3	55·0	38·7	57	34	45	49	34	41	42½	w.	w.	calm	w & n.	nw.	·313	·08	......	......	·47	41
27.	29·856	29·892	29·674	29·37	29·75	29·62	29·76	29·60	45·0	54·0	40·6	60	33	52·5	55	32	47	45	nw.	w.	calm	sw.	se.	·080	......	·54	......	·19	41
28.	29·800	29·779	29·669	29·29	29·45	29·50	29·58	29·57	49·3	54·6	43·2	54	40	47	50	41½	45	43	e.	s.	s.	sw.	se.	......	·04	......	......	·26	43
● 29.	29·712	29·729	29·677	29·30	29·59	29·79	29·82	30·10	49·8	54·6	45·4	59	43	47	59	37½	46	45	w.	ne.	calm	e.	e.	·033	......	·06	......	·21	40
30.	29·866	29·929	29·784	29·45	29·98	30·13	30·37	30·45	58·8	66·0	48·0	69	49	53	65½	43	48½	45	ne.	e.	calm	ne.	se.	......	......	......	1·60	......	51
Mean.	29·833	29·864	29·736	29·31	29·598	29·363	29·687	29·714	49·4	57·9	44·0	58·30	37·47	10·8	53·7	38·6	43·76	41·56						Sum. 1·987	1·62	1·39	1·54	4·05	Mean. 44

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SUPPLEMENT TO VOL. XXII. THIRD SERIES.

LXXXIV. *On certain improvements on Photographic Processes described in a former Communication, and on the Parathermic Rays of the Solar Spectrum.* *By* Sir J. F. W. HERSCHEL, *Bart., K.H., F.R.S., &c.; in a Letter addressed to* S. Hunter Christie, *Esq., Sec. R.S. Communicated by* S. Hunter Christie*.

DEAR SIR,

231. I BEG leave herewith to submit for your inspection and that of the Royal Society, a series of photographic impressions illustrative of the chrysotype, cyanotype, and other processes, an account of which is given in the Postscript to my last paper on that subject, which has, by permission of the President and Council, been appended to the original in its printed form subsequently to the termination of the Session. In the interval which has since elapsed, besides the discovery of other photographic novelties (which may form the subject of future communications), I have been enabled materially to improve some of the processes there described; and these improvements, with a few remarks on some other points treated of in that paper, in relation to the processes in which the thermic rays are concerned, are now subjoined.

232. The positive cyanotype process described in Arts. 219, 220 of my papers, though beautiful in its effect (especially during the first few minutes of the appearance of the picture), is very precarious in its ultimate success, owing to causes there detailed. The remedies proposed are also only occasionally and partially successful, and in consequence this process, though exceedingly *easy* in its manipulations, could not be recommended as practically useful. After trying a vast variety of means to overcome these obstacles to its success, I

* From the Philosophical Transactions for 1843, Part I., having been received and read by the Royal Society, November 17, 1842. This paper is in continuation of that concluded at p. 252 of the present volume.

have succeeded at last, by the simple addition of corrosive sublimate to the ammonio-citrate of iron with which the paper is prepared. The improved process, therefore, may be thus stated. Mix together equal measures of a saturated cold solution of corrosive sublimate, and a solution of ammonio-citrate of iron, one part by weight of the salt to eleven parts water. No immediate precipitation takes place, and before any has time to do so, the mixture must be washed over paper (which should have rather a yellowish than a bluish cast), and dried. It is now ready for use, and I do not find that it is impaired by keeping. To use it, it must be exposed to the light till a faint, but yet perfectly visible picture is impressed, and till the border (if it be an engraving which is copied) has assumed a pale brown colour. Being withdrawn it is to be brushed over as rapidly as possible with a broad flat brush, dipped in a saturated solution of prussiate (ferrocyanate) of potash diluted with three times its bulk of gum-water, so strong as just to flow freely without adhesion to the lip of the vessel. All the care that is required is, that the film of liquid be very thinly, evenly, and above all, quickly spread. Being then allowed to dry in the dark, it rarely fails to produce a good picture. And what is very remarkable, it is *ipso facto* fixed as soon as dry, so at least as not to be injured by exposure to common day-light, immediately; and after a few days' keeping it becomes entirely so, and will bear strong lights uninjured. By long keeping, details at first barely seen come out, and the whole picture acquires a continually-increasing intensity, without however sacrificing distinctness; and by the same gradations its colour passes from purple to greenish-blue. Some experience, to be acquired only by practice, is necessary to determine the proper moment for withdrawing the photograph from the action of the light. If it be over-sunned, only the darker shades appear; if too little, the whole, though beautifully perfect in the first moments of its appearance, speedily runs into an indistinguishable blot.

233. The principal obstacle in the way of the employment of gold and silver as photographic ingredients for the production of negative models, to be used for retransfers, so as to multiply positive copies, arises from the want of absolute opacity in these metals or their oxides when in a state of minute division. The same objection does not apply, or applies with much less force, to mercury, which (probably owing to its fluid state, which prevents its particles from acquiring that excessive tenuity which a laminated form would admit, by reason of their capillary forces contracting each separately deposited particle into a sphere) is one of the most opake sub-

stances (after carbon) known. I find that this high degree of blackness and opacity may be induced on a mercurial photograph prepared as in Art. 228, by a process which is in itself not a little curious and instructive, as affording a kind of parallel to the stimulating action of Mr. Talbot's second application of nitrate of silver, in his beautiful kalotype process. The nature of the process in question will be best illustrated by describing the experiment which led to it.

234. It frequently happens that papers prepared with nitrate of mercury and the ammonio-citrates or tartrates, with or without addition of tartaric or citric acid, fail to exhibit the peculiar properties described in Arts. 228, 229 at all satisfactorily. Indeed, to bring on the peculiar velvety effect there described, a high degree of intensity of sunshine seems to be an essential requisite, as, in a feeble sun, I have never obtained even an approach to it. A paper prepared (Oct. 28, 1842) according to the instructions of Art. 229 in every respect, except in the proportion of tartaric acid (which was somewhat less than that recommended), proved very little sensitive. A strip of this paper, half shaded, acquired after a few minutes' exposure to sunshine only a feeble brown colour over the sunned portion. Being then withdrawn, it was washed over with nitrate of mercury. *Immediately* the sunned portion began to darken very rapidly while the shaded part was unaffected, and ultimately assumed a deep brown hue. Exposed while yet wet to the sunshine, this passed rapidly to intense blackness, while the portion originally shaded, which had undergone the same subsequent application, and which was now equally exposed to the sun, sustained in the short time required for bringing on this effect, no appreciable change. Indeed it seemed rather to have become more insensible than before.

235. Not alone nitrate of mercury is capable of thus exciting or stimulating the dormant photographic impression on such paper. To my very great surprise, I found the same effect to be produced by *water* sparingly applied, so as only to moisten the paper. Across the sunned and shaded portions of a strip of the mercurialized paper, exposed till a pale brown was developed in the former portion, were drawn two streaks, one of weak nitrate of mercury and one of spring water. Both, after a very short interval, passed to an intense brown on the sunned half, the shaded remaining unchanged. Edging the streak produced by the nitrate was a black border, that produced by the water was uniform. The *whole* paper was now exposed for a short time to the sun, which rapidly converted to intense blackness both the streaks on the pre-

viously sunned half, while it produced no perceptible change in the other. I found this experiment to succeed on many different varieties of paper, and with very considerable latitude in the dosage of the ingredients. It was most successful in the case of a paper prepared with a cream, formed by mixing one measure ammonio-*tartrate* of iron (strength $\frac{1}{12}$*) and two saturated protonitrate of mercury, leaving out the free tartaric acid altogether, which, among many other doses of these two ingredients, proved also, generally, the most sensitive to light.

236. Led by these indications I prepared a paper by washing, first with a weak solution of ammonio-citrate of iron (strength $\frac{1}{20}$), and when dry, with saturated protonitrate of mercury. It was exposed *when barely dry enough, not to feel damp*, with an engraving in a frame to a hazy and declining sun. In about twenty minutes a very pale and feeble photograph was produced. Excited as above, by water, it gained but little in intensity (for it deserves remark that the *increase* of apparent intensity produced by either water or the nitrate, is in direct *proportion* to the force of the original impression, which, as observed, was in this case very faint). It was then held for about five minutes in the sun (near setting), and by degrees, and with the utmost regularity of gradation over every part of the picture, each line assumed an inky blackness, the lights and shades being exquisitely preserved in their due proportions, and the ground being hardly perceptibly discoloured. The result was a very beautiful and perfect negative photograph.

237. This singular power of water to excite the dormant impression, strongly recalls the analogous power of moisture to deepen the tints photographically impressed on auriferous papers, of which an instance is given in Art. 45, and of which a still more striking example is shown as follows. Let a paper be washed first with ammonio-citrate of iron, and when dry with neutralized chloride of gold, and thoroughly dried in the dark. It is then, apparently, almost insensible to light; a slip of it half exposed to sun being hardly impressed in any perceptible degree in many minutes; yet if breathed on, the impression comes out very strong and full, deepening by degrees to an extraordinary strength. Treated in the same manner, silver also exhibits a similar property†. Nor, in-

* By this I understand one part (by weight) salt + 11 water.

† Note added Dec. 21.—The excitement is produced on such paper by the ordinary moisture of the atmosphere, and goes on slowly working its effect in the dark, apparently without *other* limit than is afforded by the supply of ingredients present. In the case of silver, it ultimately produced

deed, is there any feature in photography more general or more remarkable than the influence exercised by the presence of a certain degree of moisture in favouring the action of light, whether direct or indirect.

238. There is this difference, however, in the excitement produced by simple water and by the mercurial solution, viz. that the latter is permanent, the former liable to fade; at least I have found this to be the case with the brown tinge produced by it in shade, though when blackened by a second exposure to sun no difference is perceived. On the other hand, when the nitrate is used, the brown hue frequently passes to absolute blackness without any subsequent exposure to sunshine; and in that case the photographs produced have an intensity and opacity scarcely, if at all, inferior to that of printing ink.

239. This high degree of opacity and depth, together with the comparative insensibility of the ground, is evidently capable of being most usefully applied to the production of retransfers. In fact, the photographs so produced being negative are so far fitted for the purpose, and if used as models while in this, their transition state, and as it were self-fixed, so far from being injured by the transmission of light, they are actually acquiring additional sharpness and depth by every beam which passes. By *seizing therefore the right point of dryness*, and by using a very sensitive paper to receive the impression, there is no reason to doubt of success in procuring very perfect positive transfers. Some trials I have made have satisfied me as to the practicability of this, however contrary it may at first sight appear to the usual conditions of photography.

240. In the positive cyanotype process, as improved by the addition of corrosive sublimate above recommended, we are furnished with another instance of a transformation effected by heat, analogous to those described in Art. 223. A picture prepared by this process, if heated, is transformed from positive to negative and from blue to brown. On keeping the blue colour is restored, *as well as the positive character*. In Art. 224 I have referred this curious action to certain rays,

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a perfect *silvering* of all the sunned portions. Very singular and beautiful photographs, having much resemblance to Daguerreotype pictures, are thus produced; the negative character changing by keeping, and by quite insensible gradations, to positive; and the shades exhibiting a most singular *chatoyant* change of colour from ruddy-brown to black when held more or less obliquely. No doubt also gold pictures with the metallic lustre might be obtained by the same process, though I have not tried the experiment.—J. F. W. H.

which, whether they be regarded as rays of heat, or light, or of some influence, *sui generis*, accompany in the spectrum the red and orange rays, and are also copiously emitted by heated bodies short of redness. These rays are distinguished from those of light by being invisible; they are also distinguished from the purely calorific rays beyond the spectrum by their possessing the properties recorded in Arts. 160, 223, either exclusively of the calorific rays, or in a very much higher degree. They may perhaps not improperly be regarded as bearing the same relation to the calorific spectrum which the photographic rays do to the luminous one, and if the restriction to these rays of the term *thermic* as distinct from *calorific* be not (as I think in fact it is not) a sufficient distinction, I would propose the term *parathermic rays* to designate them. These are the rays (if I may indulge in speculation which I propose to bring to the test of experiment hereafter) which I conceive to be active in producing those singular molecular affections which determine the precipitation of vapours in the experiments of Messrs. Draper, Moser, and Hunt, and which will probably lead to important discoveries as to the intimate nature of those forces resident on the surfaces of bodies to which M. Dutrochet has given the name of epipolic forces. These also, I cannot help considering it as highly probable, are the rays which radiated from molecule to molecule in the interior of bodies, determine the discharge of vegetable colours at the boiling temperature (see Art. 162), and the innumerable isomeric and other atomic transformations of organic bodies which take place at temperatures below redness. The term latent light, I confess, carries with it to my mind no distinct conception; still less capable of being introduced into scientific language appears such a term as *invisible* light. Whether the rays to which such terms have been applied shall or shall not turn out, on inquiry, to be identical with my "parathermic" rays, can only be decided by experiments to be instituted for that purpose; but at all events I feel strongly disposed to insist on the distinction between *these* rays and those of *pure heat*, and in referring them to a peculiar region of the spectrum (though without denying their more sparing distribution over every other part of it), I consider them at all events as sufficiently identified by their characters, there eminently developed, to become legitimate objects of scientific discussion.

241. The action of the calorific rays, *as such*, demonstrated by the rapidity of evaporation of water or alcohol which takes place under their influence, is traced (in addition to the facts brought forward in the notes on my first paper on this sub-

ject) in the experiments described in Arts. 205, 208, in the latter of which a chemical action, distinct from the calorific, seems also traceable. I may here also mention that the rays which operate the change of colour in muriate of cobalt from rose colour to green appear to be the calorific rays generally, and the effect to be one of simple evaporation: since under the action of the spectrum I find the green colour not restricted to the "parathermic" region, but to extend far beyond the red, and to be, in fact, commensurate with the calorific spectrum, so far as it could be traced in an experiment made under unfavourable circumstances.

I have the honour to remain, my dear Sir,

Yours very truly,

Collingwood, Nov. 15, 1842. J. F. W. HERSCHEL.

LXXXV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.*

Feb. 17, 1843.—AT the Anniversary Meeting held this day, the President, R. I. Murchison, Esq., F.R.S., announced the award of two Wollaston Medals to MM. Dufrénoy and Elie de Beaumont, for their Geological Map of the kingdom of France; and also the application of the balance of the Wollaston fund to promote the publication of Mr. Morris's tabular work, the merits of which were adverted to last year (Phil. Mag. S. 3, Vol. xx. p. 544).

The President then commenced his Anniversary Address on the progress of Geology, of which the following extracts present the most characteristic features: the paragraphs in the smaller type are our own, introduced for the purpose of connecting those in the larger, which are portions of the Address itself.

The Address begins with a brief notice of three deceased Fellows of the Society, the Earl of Munster, Dr. Arnold of Rugby, and Mr. Thomas Botfield of Hopton Court; the latter of whom amassed a considerable fortune by selecting, on geological grounds, the elevated and detached coal-field of the Titterstone Clee Hill, in Shropshire, as the seat of his mining operations. Mr. Murchison then proceeds as follows:

OFFICIAL CHANGES.

The official change occasioned by the retirement of Mr. Lonsdale having been adverted to in the Report of the Council, and the warmest thanks of this Meeting having been voted to him, I would now express my own sense of the meritorious services of that officer.

Fourteen years, Gentlemen, have elapsed since his appointment was made; during which time your collections and your volumes attest the arduous and successful labours of your Curator and Librarian. Reorganizing our Museum, and naming a multitude of species after most elaborate comparisons with foreign and British types,

* The abstracts of papers read at the ordinary meetings of the Geological Society will be resumed in a future number.

he, at the same time, undertook and performed nearly the whole of the scientific duties which were formerly discharged in great measure by our honorary secretaries; and this too at a period when currents of fresh knowledge were rapidly setting in, and when our literary machinery had been rendered much more complex than in the early days of our body, through the addition of long and well-digested Proceedings, which were chiefly prepared by him.

All these duties were executed with a fidelity and singleness of purpose, an ability, and a consummate knowledge of the whole subject confided to him, which entitle him to our deepest gratitude, and fully justify me in saying that our Transactions, Proceedings, and Collections of the last fourteen years are the real monuments of Mr. Lonsdale's labours. Alas! such efforts are more than one man can continuously sustain; and the loss of health which ensued, compelled our Curator to sever those ties by which he had been connected with us.

It is not, however, to official duties only that I must now advert; for the various works of Mr. Lonsdale, also published during the same period, prove clearly how much science might have received at his hands, had they not been bound by the trammels of official duty. His new arrangement of certain strata in the Oolitic series,—his important and original suggestion of the existence of an intermediary type of Palæozoic fossils, since called Devonian,—and his masterly description of the Silurian Corals, are alone sufficient proofs of the vigour and accuracy of his researches. Placing in him the most entire confidence, and committing to his use, for a season, the proceeds of the Wollaston fund, this Society was amply repaid by the elaborate survey of a long range of the oolitic escarpments from the south-western country, with which he had long been familiar, to the Humber—a survey, from which, I venture to say, Sir Henry De la Beche will derive the greatest advantage, when he turns his attention to these districts, in a large portion of which the boundaries of the different oolitic formations were laid down upon the Ordnance Map by Mr. Lonsdale.

The enumeration of all these duties and labours—many of them of most difficult execution—still leaves unrecorded, what every working Member of this Society must feel, that in the secession of Mr. Lonsdale we have also lost our wise and friendly adviser on every obscure and difficult point. Who among the active promoters of our science has not derived from him willing and devoted assistance? How often, when balancing difficulties inseparable from our subject, have we not benefited by his sound opinions! And with what disinterestedness and real kindness were they not offered! Where is the memoir in our Transactions, or the separate works recently published by our Members, which has not been materially improved by his suggestions? In short, I am certain I speak the sentiments of all when I say, that, from the moment of his appointment to the day of his retirement, Mr. Lonsdale infused a truly generous and highly philosophic spirit into every act and every proceeding with which he was connected. No expression, therefore, of our

gratitude can be too strong when we record his labours as a geologist, the value we entertain of his official services, and the pang of deep regret we experience in his retirement from this Society.

For a while the vacancy occasioned by the retirement of Mr. Lonsdale was not filled up; but the value which was attached to the office was attested by the fact, that nine candidates claimed our suffrage. The selection of Professor E. FORBES leads me naturally to report to you the principal geological results at which our new Curator has arrived during his recent researches in the Mediterranean seas, as they are distinctly connected with the award of the Wollaston fund during a former year, to assist him in prosecuting his inquiries in the Mediterranean or Red Seas.

Mr. Forbes observed marine tertiary strata abounding in shells at an elevation of 4000 feet in the Lycian Taurus, and he fixed the age of two distinct tertiary groups in the Greek islands. He also determined that the freshwater deposits of Asia Minor and the Sporades belong to two separate groups, the relations of which with the marine tertiary strata prove that there were two eras of submergence and elevation, in that region, during the Tertiary period. He instituted a careful comparison between the organic remains of these beds and the living inhabitants of the adjacent sea, noticing the conditions under which each are found, and thus learnt that in the newest of these tertiaries (Newer Pliocene), the remains of such species as have ceased to exist in the Mediterranean are, for the most part, at present living in the Red Sea and Indian Ocean; and hence he very logically infers, that the former conditions of the Newer Pliocene period, which imply a similar and continuous submarine area, were interrupted by that elevation of land which separated the Egean and adjacent portions of the Mediterranean from the Red Sea, the faunas of which are materially distinct from each other.

Mr. Forbes explored a submarine tract of 300 miles in width, dredging in all depths between 1 and 230 fathoms. At less than 100 fathoms, the bottom often consisted of white chalky sediment, which extended throughout the Egean, and was invariably inhabited by the same species of Foraminifera. At a depth of 200 fathoms Mollusca only, of the genera Tellina, Corbula, Arca, and Dentalium were found, but associated with Annelides, Star-fishes, Crustaceans, and Zoophytes. Lastly, he ascertained the range and characters of 500 species of existing Mollusca and of numerous associated Radiata. Among the former were species which live indifferently at all depths between 10 and 150 fathoms, and several of them (including *Buccinum semistriatum*, *Dentalium quadrangulare*, and *Pleurotoma crispata*) which had hitherto been known only in a fossil state. By this examination he also arrived at the important fact, that such species as are abundant in a fossil are extremely rare in a living state, and *vice versâ*; and thus he lays before us the last remnants of a former state of the surface of whose existence we were ignorant, accompanied by the descendants of animals, which,

first appearing in small numbers in a pre-existing period, are now attaining their maximum of numerical developement.

Such discoveries, Gentlemen, are most important to the progress of true induction; and when these researches of Mr. Forbes are presented to you *in extenso*, as is his intention, each of us will, I doubt not, find in them some illustration of the stony deposits with which we are more familiar.

I may well, therefore, congratulate the Society on having obtained the services of such a naturalist as Mr. Forbes, of whom it has been said by a distinguished foreign contemporary, that "his anatomical knowledge, the accuracy of his thought, and the vigorous precision with which he can estimate the minute differences on which the distinction of species depends, render him a worthy successor of Mr. Lonsdale, and ensure to us that he will render important services to the advancement of Geological Science*".

Having spoken of those changes in the Society which have taken place through the demise of Members and through official changes, I now proceed to consider the progress of our Science, not merely within the British Isles, but also, as far as I am able, in other parts of the world to which geological researches have been extended. In so doing I shall follow the arrangement of last year, and treat of the rocks of each country in the order of their antiquity, commencing with the most ancient. First dwelling upon the British Isles, I will next advert to Russia, the Caucasus, Asia Minor, Turkey and the Alps, and then in succession to works upon America, the East Indies and Egypt; and after an analysis of the recent progress in Palæontology, I will take leave of you with a brief *résumé* of the principal geological results.

PALÆOZOIC ROCKS OF THE BRITISH ISLES.

Silurian Rocks.—In the Address of last year I plainly expressed my belief, founded not merely on researches in the British Isles, but also on examinations of large portions of the Continent, that the Lower Silurian group contained the most ancient fossiliferous type. This view now rests upon still firmer support, established by the labours of our geologists at home, and the doubts respecting the true zoological base of the Palæozoic rocks have been entirely dispelled. In South Wales this point has been worked out with extraordinary fidelity of research, founded both on geometrical measurements and a close search after fossils; and to these investigations I will presently advert, when speaking of the labours of Sir Henry de la Beche and his assistants of the Ordnance Geological Survey.

In illustration of the structure of the Lake Country of the North of England, Professor Sedgwick has recently given a short sketch in three letters addressed to Mr. Wordsworth; and I beg you to consult this little work, which is embodied in a Guide to the Lakes published by his friend the celebrated poet, both as a specimen of the author's vigorous style of communicating popular geological knowledge, and to obtain from it a clear general perception of the

* Professor Agassiz, Letter to Mr. Murchison.

configuration of that remarkable region, and of the changes it has undergone. In regard to the older Palæozoic rocks, referring to a memoir which he read before this Society in 1832, he still adheres to the threefold division of the slate-rocks of Cumberland proposed by Mr. Jonathan Otley. With the two lowest of these, the Skiddaw slate and the green slate and its associated porphyry, we need not now concern ourselves, for they contain no organic remains. The third division, or the Upper Slaty rocks, is considered by Professor Sedgwick to represent the Silurian series. He separates it into three groups, the uppermost of which he compares with the Ludlow rocks, the second is an ill-defined hard siliceous mass, with no good fossils, and the lowest consisting of the Ireleth slate and limestone, including the Coniston, is proved by its fossils to be of the age of the Lower Silurian rocks. This view is, in short, that which has been for some time entertained by Professor Sedgwick*, and is essentially the same as that taken by Mr. James Marshall.

The labours of Mr. Daniel Sharpe, who had commenced during the preceding year a more detailed inquiry into the subdivisions of this series, are next adverted to and discussed; and after noticing Mr. Sharpe's section from the head of the lake of Bala to Dinas Mowddy and Mallwyd, and his discovery, in the Lower Silurian rocks of the Bala territory, of the *Illænus crassicauda,* a trilobite eminently characteristic of the inferior strata of the same age in Scandinavia and Russia, the President proceeds, with reference to this Lower Silurian group,—

The beds of this group are stated to rest against an unconformable mass of clay-slate forming a portion of the Berwyn chain; and to this rock, which is void of fossils, and all that lie beneath it, our author would restrain the application of the word "Cambrian."

This reasoning is clear, but the author must excuse me if I remind him that no definite base-line of the Palæozoic rocks can be established by one transverse section only, which terminates in the centre of a very complicated region. He must know, that deposits which have no existence in a given territory set on and expand in adjacent tracts. Although, then, the structure of the Lake Country naturally gives him confidence in defining the base of the Silurian system by the comparison of the Bala rocks with those of Coniston, still it remains to be proved, whether the north-western tracts of North Wales do not contain other fossiliferous bands inferior to those of Bala. As Mr. Sharpe has not examined this part of the country, the question must be answered by others; and I rejoice to say that the reply is about to be made by the geologist, who, above all others, is most conversant with that region.

You are aware, Gentlemen, that this is the very tract in which Professor Sedgwick has so long worked, and from surveys of which he gained that intimate knowledge of slaty structure, which is now considered, thanks to his masterly memoir, an essential element in practical geology. I dwell upon this point with peculiar pleasure, because I well recollect the day when the truth of those

* See Proceedings Geol. Society, vol. iii. p. 545, et seq.

lessons, which I first learnt from my friend, were opposed by many and accepted by few, though they now form part of the text-book of the field surveyor. To a re-examination of this country, then, Professor Sedgwick has devoted portions of the two last summers, with the distinct object of ascertaining, first, whether he was correct in his original opinion, on which I steadily relied, that great masses of the slaty rocks of North Wales, sometimes containing fossils, dipped under the Silurian rocks described by myself; and if so, secondly, what zoological distinction could be established between such rocks and those described as Lower Silurian. We were both aware, and the point was fully commented upon in my own work, that the Bala limestone fossils agreed with the Lower Silurian *; but depending upon his conviction that there were other and inferior masses also fossiliferous, we both clung to the hope that such strata, when thoroughly explored, would offer a sufficiency of new forms to characterize an inferior system. The results of Professor Sedgwick's recent researches would have been communicated to the Society before this Anniversary, had not his other avocations prevented his visiting London; and as the memoir will shortly be read before you, I will now so far allude to it only as to enable us to draw conclusions respecting the base of the fossiliferous slaty rocks of North Wales.

Professor Sedgwick has reassured himself that there are fossiliferous slaty masses, of great vertical thickness, which rise out from beneath the lowest Silurian rocks of North Wales hitherto described, and occupying the region of Merionethshire and Snowdonia, ultimately rest upon chloritic and micaceous schists (Menai Straits), into which they do not pass. The lowest of these fossil bands, forming the summits and flanks of Moel Hebog and Snowdonia, are, he conceives, several thousand feet below the Bala limestone.

The hope, however, which was entertained by my friend, of finding these vastly expanded and lower members characterized by peculiar groups of fossils has been frustrated, and whatever may be the thickness of this lowest palæozoic division, in which he has collected a great number of species, he now fully admits, that zoologically it is from top to bottom a Lower Silurian series †. Charged as it is with characteristic Orthidæ and Trilobites, including the *Asaphus tyrannus*, so characteristic of the lowest Silurian rock, there are, as might be expected, a few new and undescribed species; and, among these, an Ophiura (an animal whose remains had not previously been found in strata of higher antiquity than the Lias) will not appear the least extraordinary.

The base of the palæozoic deposits, as founded on the distinction of organic remains, may now therefore be considered to be firmly established; for the Lower Silurian type is thus shown by Professor Sedgwick himself to be the oldest which can be detected in North Wales, the country of all others in Europe in which there is a great development of the inferior strata. But if classification is settled, there still remains much to be done before North

* See Silurian System, p. 308.

† See an expression of the same opinions, Geol. Proc. vol. iii. p. 5. p. 549.

Wales can be as accurately laid down upon a map as the parts of South Wales to which I will presently allude; though when the operations of the Ordnance surveyors are extended to this complicated region, we shall learn, by distinct geometrical admeasurement, the exact thickness of these subcrystalline rocks on the flanks of Snowdon.

Mr. Murchison concludes this part of the subject, by expressing his dissent from a proposal made by Mr. Sharpe to strike out the "Llandeilo Flags" from British nomenclature.

Ordnance Geological Survey of England.—The progress which was confidently expected at the hands of the Ordnance Geological Survey, directed by Sir Henry De la Beche, has recently been so effectively extended to a country with great part of which I am well acquainted, that, whilst we are considering the subject of the Palæozoic Rocks, I take the opportunity to add my tribute to the large share of public approbation which such labours must earn for their authors. If my few comments on this subject involve reference to my own work, I trust the Society will believe that such allusions are made solely to explain the subsequent progress of other geologists.

In my last Address I alluded to the valuable researches of the Ordnance Geological Survey in South Wales, particularly in the great coal-basin; and I have now to speak of them amid the older rocks of Pembrokeshire and Caermarthenshire, forming the south-western tracts of the country termed the Silurian region. In the survey of that region, my chief object, as you know, Gentlemen, was simply to ascertain the general classification and right order of certain fossiliferous strata beneath the Old Red Sandstone. Having worked out the succession in typical districts in Shropshire, Herefordshire, Radnor and Montgomeryshire, I afterwards traced them to the south-west, until I equally determined their relations to the superior deposits in the coast-sections of South Wales. Although the labours in the latter country were thus auxiliary only to those of the arena on which the classification was established, I have had great satisfaction in finding, that my chief boundary lines of Old Red Sandstone and Upper and Lower Silurian Rocks are pretty nearly those which have resulted from the very systematic Ordnance Survey, the first corrected field-sheets of which Sir H. De la Beche has allowed me to view. This observation has reference only, however, to the development of what may be called one zone of Silurian rocks, or that to which, as contiguous to the Old Red Sandstone, I gave my chief attention. Of the existence of true Silurian rocks to the west and north of a certain line which was set up as a descending limit in South Wales, I was, I confess, entirely ignorant. Finding no fossils in the few visits which I made to the west of that barrier in Caermarthenshire, which was provisionally agreed upon, and to the north and west of which all the country was ultimately to be explored by my friend Professor Sedgwick, we both of us believed, that such tracts, for the most part without fossils, were of higher antiquity than the Silurian districts, and that, rising up from

beneath, they might hereafter be found to contain other and distinct forms of animal life.

The inquiry of Sir H. De la Beche has dispelled our ignorance. Instructing a number of intelligent young surveyors how to apply trigonometrical mensuration to stratified rocks, and patiently following up each mineral mass through its change of conditions upon its strike and throughout every contortion, the Director of the Survey has now clearly ascertained that the rocks to the north and west of the Towey in Caermarthenshire, as well as those to the north of Haverfordwest in Pembrokeshire*, instead of being an undefined assemblage, to which the term Cambrian had been applied, are in truth nothing but the very same Lower Silurian rocks which had been pointed out on the east and south, and which (the Llandeilo flags being much more important than the Caradoc sandstone) are repeated in great folds and undulations to the north and west. Often parting with their calcareous matter, these strata, often assuming a crystalline slaty cleavage, are in some tracts highly altered by the intermixture of trappean rocks, both of contemporaneous origin and subsequent intrusion. But in these altered rocks the Ordnance surveyors have detected true Lower Silurian fossils, and have thus, by zoological evidence as well as by geometrical admeasurements, convinced themselves that the rocks so very different in aspect, are nothing more than repetitions of the same fossiliferous strata which have been described upon the south and east. Such results, obtained amid strata so obscured by change, is one of the very highest triumphs of geological field-work; and I therefore wish to be foremost in recognising the deserts of the labourers who have obtained them, among whom the Director particularly cites Mr. Ramsay, already so favourably known to us by his geological map and model of the Isle of Arran.

In looking at the Ordnance Maps of North Pembroke, which have recently been coloured, and will shortly be issued to the public, it is surprising to see how symmetrical order has been obtained out of such a labyrinth, and how the fragments and pieces of such a patch-work are brought together. I have the authority of Sir H. De la Beche to state, that in some districts the convolutions are so rapid as to reproduce the same band of contemporaneous trap, in perfectly parallel lines, no less than ten or twelve times in the width of a mile, whilst bosses of eruptive trap are so numerous as to defy analytical research. Although I only passed quickly over the tract of North Pembroke, and ought therefore, never to have added it to those portions of my work which were more carefully executed, I have still sufficient recollection of it to admire the beauty of the new delineations. If I may be allowed to suggest a parallel between it and districts which I have more minutely described, I am greatly mistaken if North Pembroke does not present phænomena almost completely analogous to those of the mineralized Lower Silurian rocks north of Builth in Radnor-

* See Observations in Address of last year on Mr. M'Lauchlan's researches. [Phil. Mag. S. 3. vol. xx. p. 550.]

shire, and at Cornden and Shelve in Shropshire, where numerous lines of contemporaneous trap alternate with Llandeilo flags and Caradoc sandstone, and where the strata on the flanks of eruptive rocks are the seats of lead and copper ores, the sandstones being often converted into quartz rocks (Caradoc, Stiper Stones, Wrekin, &c. &c.). Combining the evidence of these tracts with those laid open by the extensive transverse sections which I formerly made in Montgomeryshire, in the north-western parts of the Silurian region, where the masses have been shown to roll over in great undulations from S.E. to N.W., I am fully prepared to admit the existence of a similar configuration in North Pembroke, West Caermarthenshire and Cardiganshire, districts with which I was very imperfectly acquainted, and where the aspect of rocks is at first sight, it must be admitted, very forbidding to those who search after fossil evidences. The greater, however, the difficulty, the greater is the merit of those who have solved the problem, and have thus established in parts of South Wales the precise relations of what were previously considered to be anomalous masses. The result of this Survey, up to the present moment, is, that in one small part only of North Pembroke is there any development of rock older than the strata containing Lower Silurian fossils, and this occurs in the promontory of St. David's, with which I am familiar: this rock, I can confidently say, is mineralogically undistinguishable from the close-grained purple greywacke of the Longmynd and Haughmond Hill in Shropshire; and in both these localities it has hitherto been found as void of fossils as in the similar rocks of the Lammermuir Hills in the South of Scotland.

In the south-eastern parts of the Silurian region, to which the Ordnance Geological Survey has also extended its labours, the accuracy of the chief lines which had been laid down, whether in May Hill, Usk, Woolhope, and the Malvern Hills, has been confirmed; and, under the vigilant eye of Mr. Phillips, some new species have been added to the former lists, both in the Lower and in the Upper Silurian rocks. Among the latter the *Pentamerus Knightii* has been found in a new locality, in the southern prolongation of the axis of Woolhope, thus showing how persistently the place of the Aymestry limestone is maintained; whilst a species of that remarkable shell, the Pleurorhynchus, has been detected in true Wenlock limestone.

In relation to the west flank of the Malvern Hills, Mr. Phillips has, by very close researches, come to an important conclusion. Certain specimens of a peculiar conglomerate or breccia having been found by his sister Miss Phillips, in which the *Pentamerus lævis* and other Caradoc fossils are associated with fragments of syenite, a further search was instituted, and a small boss of this rock was laid bare on the very edge of the syenite and in a vertical position, like most of the beds of the same formation along the north-western prolongation of these hills. The conclusion drawn by Mr. Phillips is, that a portion at least of the crystalline Malvern chain was in existence when the Caradoc sandstone was formed, an infer-

ence which is strengthened by the finer-grained adjacent and regularly-bedded varieties of the sandstone containing similar minutely triturated, igneous materials. At the same time it is certain, that the great upheavals of the syenite and trappean rocks took place long after the deposition of the Silurian strata, and even after that of the old red sandstone and coal-measures, which at various points along this ridge, and particularly at the Abberley Hills, have been violently dislocated in contact with such intrusive rocks. The discovery on the west of the Malverns is, however, analogous to what has been observed along the flanks of the granitic axes in the Highlands of Scotland (Ord of Caithness, &c.), where fragments of rocks derived from them are imbedded in the old red sandstone conglomerates, thus showing an original crystalline nucleus, followed by other granitic eruptions. The Isle of Arran offers proof of such a period of activity, which it has been inferred, was posterior to the contiguous red conglomerate, in which no granitic fragments are imbedded.

When he pursues his researches to the northern parts of the Silurian region, Mr. Phillips will then see, on the flanks of the Breidden Hills, evidences nearly analogous to those which he has so well described in the Malvern Hills, and where it has been shown, that along a very ancient fissure of eruption, molten matter was consolidated before the existence of the Silurian rocks; that other eruptions followed, and were in continuous activity during the formation of the Lower Silurian strata; that again other upheavals took place by the rise of intrusive trap, which threw the previously-formed contemporaneous plutonic deposits upon their edges; that the coal-measures deposited unconformably on such uplifted strata were afterwards deranged; and finally, that along the very same line of eruption, igneous matter, undistinguishable in mineral composition from that which had affected the ancient rocks, has cut its way in irregular dykes through the new red sandstone, and, from the isolation of a deposit of lias, was probably ejected subsequent to the accumulation of that deposit*.

Such facts are, it seems to me, miniature counterparts of the upraising at successive periods of mountain chains; and the grand phænomena of the Caucasus, the Alps, and the Pyrenees may nearly all be studied in our small English ridges, and some of them peculiarly well upon the flanks of the Malverns, and their continuation the Abberley Hills.

In the sequel I shall have occasion to speak of other important researches of Mr. Phillips. For the present, then, I take leave of the Ordnance Geological Survey, assuring this Society, that having during the past year, for the first time, seen the practical application of the admirable method of field-survey which has been instituted by Sir Henry De la Beche, I am convinced that it will not only act directly as a great national benefit, in making more correctly known the structure of the subsoil, in a manner beyond the reach of private

* See Silurian System, p. 294.

enterprise, but that it will materially tend to elevate Geology, by connecting it in a permanent manner with Physical Science.

"*The Rocks of the Scottish Border,*" and Mr. W. Stevenson's memoir on the Geology of Berwickshire are in the next place treated of; and the President then proceeds to notice the

Irish Ordnance Geological Survey—Tabular List of Irish fossils.—A compendious volume, entitled 'A Report on the Geology of Londonderry, and parts of Tyrone and Fermanagh,' has just been published by our associate Captain Portlock, R.E., employed in the Irish Trigonometrical Survey. Illustrated by a geological map, numerous coloured sections, and plates of organic remains, this closely packed volume, of nearly 800 pages, is a sample of how great a mass of matter may be derived from a small district. Not having had sufficient time to study the details of this work, I must crave the author's indulgence if I refer only to such parts of it as have arrested my attention. Captain Portlock, having some time ago discovered a small patch of Silurian rocks in the region of his official labours, commenced a careful and systematic inquiry into the nature of the Trilobites with which it seemed to abound, and he now presents us with some very valuable results. In a preliminary discourse he offers many important remarks upon the affinities and anatomy of this group of animals, and after a very elaborate comparison of all the forms which he could detect in his district with those published by British and foreign authors, citing among the latter several works very little known to us, he arrives at the conclusion, that of sixty species in this palæozoic tract, fifty-two belong to true Silurian strata (for the greater part Lower Silurian), and eight only to the enormously developed carboniferous limestone of the North of Ireland. This fact is quite in accordance with what has long been my belief, that the Silurian or oldest palæozoic group is the great centre of Trilobitic life. Describing many new forms, which are figured, he establishes several new genera, among which the Rhemopleurides, obtained from the Lower Silurian rocks, is a very curious and apparently quite distinct trilobite. There are but small traces of Upper Silurian or Devonian deposits in this district, the greater part of it being covered by a carboniferous series, consisting, as the mountain limestone of Ireland is known to do, of much sandstone and slate as well as limestone. Finding in it several shells which are eminently characteristic of the lower as well as of the upper beds of that great formation, he infers, and I think with perfect justice, that the mountain limestone of the North of Ireland must be compared with the whole and not with the upper part only of that formation in the North of England; an opinion I am prepared to support, by having found last summer several shells (notably the *Sanguinolaria undata*), which are published by Captain Portlock from the north of Ireland, in the very bottom beds of the limestone of Berwickshire and Northumberland. Confining myself to the researches of this author in the palæozoic rocks, on which he has shown so much skill, I must also request my hearers to consult this volume of the Irish Geological Survey, for

much information respecting the overlying strata, among which some new features of the Keuper formation are sketched with the author's usual fidelity.

From the researches of Captain Portlock I turn to those of Mr. Griffith, who spent many years in preparing the Geological Map of Ireland, and for which he has deservedly received much praise. In a very elaborate comparative table of the fossils of the mountain limestone series of Ireland, presented to the Manchester Meeting of the British Association, Mr. Griffith divides that series into five subformations, which in ascending order are the Yellow Sandstone, Carboniferous Slates, Lower Limestone, Calp, and Upper Limestone. He also shows that the two lower of these subdivisions must, from their fossils, many of which ascend into the overlying strata, be classed with the mountain limestone series, and not with the Devonian rocks; in which case, I would observe, that they must also be classed with the sandstone, limestones and shale of Berwickshire, to which allusion has already been made. I am the more induced to believe in the accuracy of this comparison, because Count Keyserling and myself have this year confirmed the observation of Professor Sedgwick, made in 1828*, viz. that Posidoniæ, similar to those in the culm limestones of North Devon, exist in the middle of the mountain limestone series of Northumberland. As Mr. Griffith has shown that the Irish Calp, which also occupies the middle place in the limestone of Ireland, contains the same peculiar fossils, the parallel may now be considered as very well established, between this central mass of the mountain limestone in these distant localities.

Drawn up as this table has been under the directions of Mr. Griffith, by a diligent young naturalist of good promise, Mr. F. M'Coy, there can be no doubt that it is entitled to much consideration, and that its publication will be very useful. In reference to the comparison instituted by Mr. Griffith between the strata of North and South Devon and those of Ireland, I may observe that it is of infinite importance to the establishment of a true series of equivalents, that large adjacent tracts of country should be surveyed, and their fossils compared by the same observers, for the want of which identical species may sometimes obtain different specific names; thus considerably interfering with a nice discrimination of the groups.

Palæozoic Fossils.—Before I quit the subject of the older British rocks, it is my duty specially to call your notice to a memoir just published in your Transactions, because, although inserted by order of the Council, and throwing great light on British palæozoic remains, it has not yet been sufficiently alluded to from this Chair. It is the work of MM. de Verneuil, and D'Archiac on the Fauna of the Palæozoic Rocks. In the first instance this memoir was designed to consist simply of a description, by M. E. de Verneuil, of the organic remains of the Rhenish provinces explored by Professor Sedgwick and myself, and of which you have now our published views. M. De Verneuil, who combines the attainments of a good conchologist

* See Professor Sedgwick and Mr. Murchison, Geol. Trans. vol. iii. p. 693.

with those of a geologist, had accompanied us during a part of our survey of that region, and approving the general classification, had kindly offered to illustrate our views by the use of the very fine fossils from the Rhine and the Eifel which his cabinet contained. While instituting the comparisons necessary to prove, by evidences independent of those in England, that the Devonian is a true intermediary type, the subject became enlarged in his hands, and he was so fortunate as to procure the assistance of his friend the Vicomte D'Archiac. By combining researches and making a variety of comparisons, their work soon acquired great value, not merely as regards an accurate description of beautiful organic remains, admirably lithographed, but as containing also a general tabular list of the fossils of the Devonian system in Europe, as compared with the species of the Silurian and Carboniferous deposits in the Rhenish provinces.

This table, enriched as it has been up to the moment of publication by additions drawn from recent researches in Russia, must be considered a standard acquisition, the intrinsic merit of which can only be estimated by those who are aware of the labour and range of study which its preparation required. Nor are the general views of the authors, which are embodied in their preface, less worthy of consideration, for they exhibit an intimate acquaintance with fossil zoology and its relations to each great system of the palæozoic rocks; whilst it must be satisfactory to British geologists, that the inductions of these foreign naturalists are in harmony with those of Mr. Phillips, drawn from his own observations upon the fossils of Devon and Cornwall. As this is the first occasion on which French geologists have presented to us a memoir illustrating the writings of our own countrymen, I am confident you will unite with me in thanking them most cordially for their liberal and enlightened assistance. Whilst considering the palæozoic classification established in Great Britain, I have, therefore, thought it not irrelevant thus to allude to the labours of foreign geologists which support it; and I have the pleasure of acquainting you, that in addition to their claims upon us, MM. D'Archiac and de Verneuil have, during the last summer, explored parts of Normandy and Brittany, where they have clearly recognised in that obscure tract, the same palæozoic divisions as exist in the Rhenish provinces and the British Isles.

I cannot take leave of the palæozoic rocks of our own islands without communicating to the Society a fact, which minute as it may seem to be, is of interest in regulating our views respecting the development of animal life. The Rev. P. B. Brodie, to whose researches in the secondary rocks I shall presently allude, informs me, that he lately discovered in the Silurian limestone on the west flank of May Hill, Gloucestershire, two palates of fishes. Now as the rock in question is of the age of the Wenlock limestone, we learn that fishes existed in the inferior as well as in the superior member of the Upper Silurian rocks, in which they had previously been noticed. No trace, however, of Vertebrata has yet been discovered in the widely extended and enormously thick Lower Silurian deposits.

Igneous Rocks of South Staffordshire.—As connected with the older depositary rocks of the central counties of England, I will now direct your attention to some recent observations on the changes to which they have been subjected by igneous agency. Besides forming a most instructive museum, singularly rich in Silurian and Carboniferous species (including many unpublished), the Geological Society of Dudley, to the establishment of which I last year alluded, has produced a report 'On the Igneous Rocks of the South Staffordshire Coal-field*,' not yet printed, with the results of which I am convinced you will thank me for making you acquainted. In the southern portion of this coal-field there are centres of eruption where the basaltic and trappean matter, rising from the bowels of the earth, completely cuts through the surrounding carboniferous strata, dislocating and altering them in the manner which has been pointed out by Mr. Keir, Mr. Arthur Aikin, and other observers, including myself. In regard to the lateral injections of the igneous matter among the coal strata, Mr. Blackwell, by comparing the shaft-sections of contiguous collieries, has shown, that however a single vertical section might seem to afford grounds for belief, that such igneous matter had been formed as a bed, yet that in reality it traverses various depositary strata in a slightly oblique direction, and often thins out in the form of a wedge. From such apparently horizontal masses, vertical dykes, with occasional lateral veins of white felspathic rock, varying in thickness from a few inches to three or four yards, are seen to rise up and traverse the coal-beds in the most irregular manner, though such dykes are not found to produce any derangement in the regular measures. Another good fact observed in relation to a supposed horizontal sheet of basalt in the midst of the sedimentary strata, is, that the beds on which it rests are less subject to faults and dislocations than those which lie above it; the intruded mass, indeed, sometimes rising up to the surface and forming a knoll, occasionally bare, and at other places covered by the coal strata, which mantle round it.

The report also proves, that some of the chief faults which radiate from one of the great centres of eruption (Barrow Hill), were produced contemporaneously with its elevation; since the basaltic matter which flows laterally in apparent beds, follows the line of fault, bending and leaping up, as it were, without a break from lower to higher levels. From this fact Mr. Blackwell infers, that the basaltic matter must have been in fusion when it was extruded laterally along the surfaces of certain strata, but that the subsequent dislocation by which these beds were moved to different levels, took place before the igneous matter had cooled and when it was still plastic. This very remarkable phænomenon, which is indeed similar to examples cited by Dr. Mac Culloch, is an exception to the cases in other parts of the district (Wolverhampton), where the beds of basalt are broken off and change their relative places with the coal strata; and thus we learn

* The Report to which allusion is here made, including the Sections, was drawn up by Mr. J. H. Blackwell of Dudley, assisted by Mr. W. Spencer.

that in the same district the faults were not all produced at one period.

Near Wolverhampton, the underground trap, which is spread over an area of five miles by two, is to a great extent perfectly conformable to the coal-measures above and below it; but when traced for upwards of three miles, its nonconformity to the coal-beds becomes apparent, and this is further confirmed by the giving off of white vertical felspathic dykes, similar to those before alluded to. It is, however, worthy of record, that no centre of eruption has yet been discovered in this part of the district whence the flow of basaltic matter could have been derived, though it is believed that it may have proceeded from the distant Rowley Hills.

After minutely elucidating the distinctions and the variations of structure and form in the various trappean rocks of this district, the report proceeds to point out the changes which have been produced in the coal-measures by their intrusion. The same beds of coal, which in parts of the field exempt from basalt are highly bituminous, are, when in contact or in the vicinity of that rock, either converted into anthracite or charred into cinders. In the Wolverhampton tract the upper portion of the coal beneath the greenstone is entirely cut out, whilst the lower part is converted into anthracite, and often fissured by vertical joints, accompanied by veins of calcareous spar; as if, in driving off the bituminous matter of the coal, the extreme heat had also occasioned a contraction of the carbonaceous mass. Besides these alterations of the coal, the beds of ironstone are equally affected, the clay being rendered porcellaneous, and the sandstones, usually much hardened, are in some cases even vitrified.

In addition to these changes, which are analogous to effects observed in other countries, and are such as a high degree of temperature will readily explain, other alterations are cited, with some of which, indeed, Mr. W. Matthews acquainted me when I examined the tract, and the solution of which is not so easy; viz. that beds slightly impregnated with iron at a distance from them, become gradually more charged with it as they approach the igneous rocks, and are of very superior quality in their immediate vicinity. This fact, indeed, is perfectly in accordance with what I have lately observed in the Ural Mountains, where masses of iron ore are crystalline and even highly magnetic when in contact with eruptive rocks.

Another point dwelt upon, perhaps still more curious, and which also requires the consideration of the chemist, is, that wherever igneous rock is present in the neighbourhood of beds of coal, and yet separated from them by an intervening substance, which has prevented their being injured by intensity of heat, the coal is frequently what is called "brighter and more bituminous!"

With such facts before them, the authors of this report are aware, that other agencies than those of mere heat, are required to account for the production of iron concretions and the crystalline structure of coal; but whilst they think that electric and magnetic currents must also have operated in bringing about some of the results, they are convinced that the presence of igneous matter, often ex-

tended laterally in the form of vast beds, must have had a great share in the production of such phænomena. Having myself taken no little interest in the formation of the Dudley and Midland Geological Society*, I hail this report upon the physical condition of the subterranean masses of that tract as worthy of the approbation of men of science in all countries; for the history of Dudley is that of many regions of the earth, which have been penetrated by intrusive matter.

The progress made in our knowledge of the SECONDARY BRITISH ROCKS, under the several heads of *New Red Sandstone, Lias and Oolites, Wealden* and *Cretaceous System,* is next stated, leading to the

TERTIARY PERIOD.

The session has not added much to our knowledge of British Tertiary deposits. Mr. Trimmer, well known to us by his researches in detrital phænomena, has lately expressed his opinion that the peculiar eroded surface of the chalk, in which pipes filled with sand or gravel are of frequent occurrence, was produced by the action of the sea during a period which preceded the deposit of the London clay. There can be no objection to this view being applied to all those corroded surfaces of the chalk, which are surmounted by the Eocene deposits of plastic clay and sand and London clay, including, I would add from the recent observations of Count Keyserling and myself, the junction of the chalk and Lower Tertiary in Alum Bay. It would, however, be manifestly wrong to suppose, that such a corrosion of the surface of the chalk had not also been effected at other and subsequent periods; and as proofs of a still more recent corrosion, the observer has only to examine the shore and cliffs near Brighton, and see how similar cavities have been filled up by a breccia, in which the bones of elephants are imbedded.

Some remarkable concretions in the Tertiary beds of the Isle of Man (where the newer marine Pliocene strata were first described by Professor E. Forbes, and shown by him to occupy perhaps a larger area than in any one locality of the British Isles), have elicited from Mr. H. Strickland the suggestion, that they were caused by currents of water, or by the action of wind during ebb-tide.

Among the terrestrial phænomena which have recently excited notice, is the discovery by Dr. Riley of a bone-cavern in the Mountain Limestone of Durdham Down near Bristol, the opening of which has been conducted by Mr. Stutchbury, who has described its contents. Distinguishing, as Dr. Buckland had formerly done, the cavities formed by fissures in the rock, into which bones had been washed with detritus of rocks and soil, or into which whole animals had fallen, from caverns inhabited by extinct species of canine animals, Mr. Stutchbury shows, that the facts observed in this case entirely favour the latter hypothesis, the bones (among which those of the hyæna vastly preponderate) being fractured into small bits without the admixture of any rolled or far-transported detritus. The most

* See Address to the Dudley and Midland Counties Geological Society, 1841.

novel point connected with this cavern is, that several of the hardest bones and teeth have been split across, and their parts relatively moved, as if the detrital mass had been affected by faults posterior to its original deposit, which movements may, Mr. Stutchbury supposes, have been connected with the operations which closed the orifice of the aperture*.

The President now proceeds to the subjects of *Geological Dynamics, Detrital Phænomena, Glacial Theories, Raised Beaches and Shingle Terraces, Marks of Ancient Levels of the Sea*; noticing in succession the labours and views of Mr. Hopkins, Prof. Sedgwick, Mr. Scott Russell, Mr. Clay, M. Agassiz, M. Charpentier, M. Hugi, Prof. Keilhau, M. Elie de Beaumont, and especially those of M. Bravais.

Russia and the Ural Mountains.

Having occupied your attention during the past session with memoirs on the Geological Structure of so large a portion of the earth as the Russian empire, I must make a few allusions to a subject which has to a great extent engrossed my thoughts and those of my coadjutors, M. E. de Verneuil and Count A. Keyserling. Employed as we are in preparing a work explanatory of our views, in which we hope to do justice to all previous inquirers†, and to the Imperial Administration of Mines which supported us, I will not on this occasion venture to occupy more time than will be sufficient to touch upon some of the most striking geological features of that empire, which either sustain or enlarge our views of classification and comparison.

Silurian Rocks.—The Silurian, Devonian and Carboniferous deposits of Russia, are each characterized by distinct organic remains; and these rock systems are very clearly separated from each other over enormous spaces. Occupying (including the Baltic islands) a tract as large as the principality of Wales, the Silurian rocks, like those of Norway and Sweden, are unequivocally the oldest fossiliferous strata, since they are seen to repose upon the primary crystalline masses of Finland and Lapland. Little elevated above the Baltic Sea and the rivers of the northern watershed of Russia, these Silurian rocks constitute low plateaus only of limestone, clay and sandstone, often incoherent, and on the whole of very small thickness; thus exhibiting the most obvious contrast to their mountainous and frequently sub-crystalline equivalents in Western Europe and the British Isles. In their small vertical dimensions they present to us, indeed, a very instructive lesson, for in passing from Norway, Sweden and Gothland into Russia, the distinguishing strata thin out, and losing their divisionary lithological characters, part also with many of their characteristic shells. "When followed from one region to another, deposits of all ages exhibit like contractions and expansions, dependent on the forms of the ancient bays, the nature of

* A similar case occurred in the gravel beds of Darmstadt, where the Dinotherium was found.

† During its preparation, our general map of the Russian empire has been much improved in its north-eastern extremity, the country beyond Archangel which we did not visit, in consequence of the observations of the distinguished botanist M. Ruprecht.

the springs and currents, and the depth of the seas in which they were accumulated *."

Devonian Rocks.—If the horizontal range of the Silurian rocks of Russia be considered large as respects our terms of comparison, what will my associates say to the expanse over which the Devonian or next ascending group is spread, when I tell them that it is much larger than the whole of the British Isles? Reposing upon the low Silurian plateaus, this widely ranging deposit rises to heights of from 500 to 900 feet above the sea; and it is very remarkable by being charged in many localities with ichthyolites, several species of which, hitherto considered peculiar to the Scottish Old Red Sandstone, are found associated with Mollusks, perfectly similar as a group, and often specifically the same, as those of the limestones of South Devon, the Boulonnais and the Eifel. The discovery of the intermixture of Scottish Old Red Sandstone fishes and true Devonian shells in the same strata, was, you may believe, one of the most gratifying results of the recent explorations in Russia, as being confirmatory of the views of Professor Sedgwick, Mr. Lonsdale and myself respecting the divisions and equivalents of that member of the Palæozoic Rocks. In some parts of Russia, the Devonian rocks are red sandstones and marls; but in an extensive central tract, where they rise into a dome which separates the northern from the southern basin of the empire, they are composed of yellow sandy marls and limestones, which lithologically might be mistaken for the magnesian limestone beds of our northern counties:—so inapplicable are mineralogical terms as marks of geological epochs.

In the vastness of their undisturbed and nearly horizontal extent, these strata afford us most instructive proofs of the intimate connection between the stony condition of the rocks and the imbedded fossils; for, when the calcareous matter is present, various mollusks are associated with some fishes, whilst in those tracts where the limestones disappear and the beds have the characters of the Old Red Sandstone of Scotland, fishes only can be detected: thus presenting a remarkable analogy between the distribution of this very ancient fauna and that of existing nature; the present great receptacles of fishes being deep sandy bottoms, whilst shelly creatures congregate towards the shores where calcareous springs attract them. I shall elsewhere allude to the fishes of this deposit when speaking of the researches of Professor Agassiz.

Carboniferous Rocks.—The Carboniferous deposits, which succeed, cover an area as broad as that of the Devonian or Old Red rocks; and they are throughout clearly distinguished by a decidedly distinct type of animal life, presenting in some families an extraordinary number of species absolutely undistinguishable from those of our own country published in the works of Sowerby and Phillips. This system is eminently calcareous, and exhibits a vast marine succession, in which the fossils of the Mountain Limestone

* Geological Researches in Russia (in the press) by Roderick Impey Murchison, E. de Verneuil, and Count A. von Keyserling, assisted by Lieut. Koksharof, p. 40.

prevail, whilst in no part of the empire is there a trace of the overlying deposits, with which we are familiar under the term of "coal-measures." Coal, however, does occur at intervals, both underlying the carboniferous limestone, as in Berwickshire, and alternating with its central and upper members, as in Northumberland, and the carboniferous valleys of our lake country.

Of the latter, the extensive tracts in the south of Russia, occupying from 10,000 to 11,000 square miles, and usually known as the country of the Donetz, offer a very striking illustration; containing in one district as many as seven good seams of coal, subordinate to sandstone, shales and limestones, very analogous to those rocks which abound in the western dales of Yorkshire. These strata, based upon the crystalline rocks of the southern steppes, constitute a greatly disturbed region, and, owing to numberless convolutions, present the most remarkable contrast to the horizontal deposits of Central and Northern Russia. It is therefore difficult to observe the order of succession; but, owing to our previous acquaintance with the types in their normal condition, we were enabled to trace the sequence, from conglomerates and sandstones at the base of the carboniferous limestone, up to the equivalents of the Magnesian Limestone or Zechstein.

Whilst thus briefly alluding to this tract, I must pass, for a time, from my own labours, and those of my friends, of the value of which you must judge when our work is completed, in order to mention the recent appearance of the fourth volume of the splendid work of M. Anatole Demidoff, 'Voyage dans la Russie Méridionale,' which is entirely devoted to the description of the carboniferous region of the Donetz. M. Le Play, an eminent French engineer, happily selected by M. Demidoff to ascertain the true mineral wealth of the tract, and to describe its physical and geological structure, has produced a work so replete with well-digested details, collected, not only from observations of the natural features of the region and the mines which have already been commenced in it, but also by numerous borings carried on by himself or his assistants during a period of three years, that the Imperial Government will doubtless feel grateful to the accomplished patron who has so liberally fostered these inquiries.

In a large geological map, in which the demarcations of the carboniferous and crystalline rocks, and also of the overlying secondary and tertiary deposits are given, M. Le Play has grouped under darker colours such parts of the tract as are known to be productive of coal, to distinguish them from those in which the mineral has not yet been discovered. This method, doubtless, carries with it a certain amount of information, but is deficient in stratigraphical meaning, for some of the beds so marked are higher than others; in some the coal is interlaced with limestone, and in others it is almost entirely subordinate to sandstone and shale; in one tract anthracite exclusively prevails, in another bituminous coal. By reference, however, to the explanation, and to a series of tables, this defect is obviated. These tables are, in fact, perfect models for the

practical mining engineer; they give at one view the direction, inclination, thickness and quality of the coals at each locality, also the characters of the associated strata, as well as the state of the works, and their produce at each mine or trial-spot. To these is added another set of tables, in which the chemical analysis of the coals from forty-three different places is given by M. Malivaud, another agent of M. Demidoff.

Into such details, valuable as they are, it was not our province to enter, and I will now, therefore, merely offer a few remarks explanatory of those points in which the geological conclusions of my friends and self either agree or are at variance with those of M. Le Play.

Certain fossils which he had brought to France, and which we inspected before our journeys to Russia (1840), first led us to believe, that these coal beds are subordinate to the carboniferous limestone. Of this, indeed, there could be no doubt, for the species were, to a great extent, the very same as those with which we were familiar in rocks of that age in western Europe. On interrogating M. Le Play, however, we could not ascertain that he had arrived at any defined idea of a succession of strata, derived either from the stratigraphical order of mineral masses, or from their imbedded organic remains. In fact, he then distinctly acquainted us with what has now appeared in his work, that, owing to the disturbed and convoluted condition of the strata, the want of persistency of mineral characters, and the apparent existence of similar species of shells throughout the series, it was impracticable to assign a base line to the deposits, or to trace their uppermost limits, still less a passage into any superior formations.

Now, as we have ventured to effect these objects, with what success we must leave others to decide, I will here briefly state why I conceive M. Le Play did not arrive at similar results; although he had in his own hands some means of proof, which, through the short time at our disposal, we never obtained.

No geologist, however practised, can, I venture to say, explain the structure of any complicated part of a distant country, unless he has made himself master of the clear succession of its normal formations. Long as I have been occupied in the study of the Palæozoic rocks, I am confident that, had my friends and myself been thrown suddenly into the chain of the Donetz, and had been desired at once to unravel its complexity, we should have reached no other geological result than that to which M. Le Play has attained, viz. of stating that the coal-seams are, as a whole, subordinate to the carboniferous or mountain limestone. We had, however, by two years of extensive comparative researches, obtained an intimate acquaintance, not only with the older Palæozoic rocks of Russia generally, but, in reference to the carboniferous system, had convinced ourselves, that, throughout the enormous area over which we had traced it, the upper or coal group of western Europe was absent; and that the calcareous or lower group, occupying the whole carboniferous horizon, was divisible into three stages, by help of certain fossils characteristic of each. Again, we had ascertained, by numerous

sections on both flanks of the Ural Mountains, that, in becoming part of a mountain mass, this system, so uniform and so peculiar over a space as large as an ordinary European kingdom, put on many of the features which are so well known to those who have studied the carboniferous limestone only in the western parts of Europe.

We further learnt, that, in the absence of any deposits to represent our great coal-fields, the carboniferous system was succeeded, in ascending order, by a vast series of red and cupriferous deposits to which we have assigned the name of Permian. It will not, therefore, be arrogant on our part to say, that we entered upon the examination of the territory of the Donetz, in the possession of elements of comparison which no previous travellers had acquired.

Knowing, from the maps and instructions furnished to us by the Imperial Administration of Mines*, that the major axis of this tract and the main direction of the strata trend from west-north-west to east-south-east, we resolved, after terminating our researches in Southern Russia, to examine the chain of the Donetz in parallel lines transverse to its general strike; and, by carrying out this scheme, we arrived at the conclusion, that the oldest member of the series occupies its southern frontier, and that, after a multitude of flexures, the central strata dip under a limestone charged with Fusulinæ, fossils which we had invariably found in the uppermost bands of limestone; the whole group being surmounted in the valley of Bachmuth by the equivalents of the Permian system. One striking deficiency, however, attached to our reconnaissance, and fortunately it has been supplied by M. Le Play himself. Those members of the Society who heard our memoirs read, will recollect the importance we attach to the presence of the large *Productus giganteus*, as uniformly characterizing (over vast regions in Russia) the lowest beds of the carboniferous limestone; and, as we now learn from M. Le Play, that this fossil, of which he collected many individuals, occurs in the southern part of the region, our idea is thus completely confirmed of an ascending section from south to north.

In fact, the examination of the carboniferous region of the Donetz is one of the best examples that can be adduced, of the paramount importance to the practical miner of the close study of organic remains, in reference to the normal position of the strata; for, throughout deep sections in the northern part of the same territory, there is not a trace of this great Productus, whilst all the fossils of the middle and upper strata are present. Any one, therefore, who had felt as confident as we do, that this remarkable fossil was as clear an indication of a lower band as the *Spirifer Mosquensis* and Fusulinæ are of an upper, could not have doubted of the general relations and order of the strata in the chain of the Donetz.

* The instructions of General Tcheffkine, the works of Captain Ivanitzki, and a map by Colonel Olivieri. See Journal des Mines, &c.

Agreeing in the correctness of the general parallel which M. Le Play has drawn between the deposits of the Donetz and the carboniferous limestones of Great Britain, Belgium and France, I do not believe that beyond this point his comparisons can be sustained. The coal fields, for example, of the Low Countries and of Düsseldorf, with which I am well acquainted, do not offer, as he supposes, an analogy to those of the Donetz; for in the former, coal-seams are in no instance interstratified with the Mountain Limestone series of English geologists, but are invariably superposed to it. Again, in the Prussian and Belgian provinces, the mountain limestone with sands and shale but void of coal, reposes on a fine succession of Devonian and Silurian rocks, loaded with typical fossils; whilst the group of the Donetz, exclusively carboniferous to its base, rests at once either on very ancient crystalline rocks, or upon porphyries and other eruptive masses, to the agency of which is to be attributed the extraordinary contortions into which the strata have been thrown. The true analogy, therefore, of the coal of the Donetz, considered in reference to other deposits of the same age, is to be found in the north-western English districts of mountain limestone, in which several workable seams occur; though in this comparison it must be stated, that the Russian beds contain much more coal than the British strata. But even if we admit that, to some extent, there is a similarity in the carboniferous rocks of South Russia and the Low Countries, in their being both flanked by cretaceous deposits, we must also not omit to recognise a great discrepancy, through the presence in the one case of overlying strata of the age of the Zechstein, and in the other by the total absence of that deposit.

To considerations of theoretical importance concerning the changes which the surface of Southern Russia may have undergone, and which are ably put forth by M. Le Play, I will not at present advert; reserving my views on these points for the concluding chapters of the work upon Russia, when all the elements which my friends and myself can bring together shall have been laid before our readers, to enable them to see the grounds upon which the conclusions are based.

For the present, then, I take leave of this volume of M. Le Play, which, though it contains some views of positive geology from which I differ, must still be regarded as an important addition to the records of physical science, and as possessing much more the character of a good monographic description of a given tract in Russia, than anything which, from the extensive nature of our researches, my friends and myself will ever be enabled to offer.

Permian Rocks.—On its eastern frontier, far removed from the tract to which allusion has been made, the uppermost member of the carboniferous limestones of Northern and Central Russia, distinguished by the presence of multitudes of the foraminifer *Fusulina*, is succeeded by the most widely spread of the Russian systems; to which, from its occupying the whole of the ancient kingdom of Permia, we have assigned the name of Permian. You have been

told, that this vast group is composed of limestones, marls, great masses of gypsum, rock-salt and repeated alternations of cupriferous strata; and that it contains a flora and a fauna of characters intermediate between those of the Carboniferous and Triassic periods. The shells are, to a great extent, those of our Magnesian Limestone or Zechstein; and, like the conglomerate of that deposit near Bristol, the Permian rocks are distinguished by the presence of Thecodont Saurians. The interest attached to these vast deposits, which have been spread out on the western flanks of the Ural Mountains, is increased by the inferences which have been drawn, that springs and currents holding much copper in solution must have flowed from the edges of that highly mineralized and metamorphic chain, while the Permian strata were accumulating. But the great value of having worked out a fuller and richer type of a group of strata between the Carboniferous and Triassic epochs than any which exists in Western Europe, will be found in the fossil shells, the plates of which are already far advanced; for, with some species hitherto known in the Zechstein of Germany and Magnesian Limestone of England, we shall publish others which are identical with or analogous to forms that occur in rocks occupying the same geological position in North America, of which I will speak hereafter, and concerning the age of which great doubts had prevailed.

In America, indeed, as in Russia, these beds had been compared with every deposit, from the coal to the Keuper inclusive, whilst in our work they will be shown to have no connection with the New Red Sandstone or Triassic group, but to occupy a definite position, truly intermediate between that system and the carboniferous. At the same time it is manifest, that although they overlie and are, as they ought to be, very distinct from the Carboniferous system, yet they contain some species of shells which occur in that division. Thus it will be made evident, that after all there now remains scarcely any real difference of opinion on this head between Mr. Phillips and myself (to which I alluded last year); for I learn from him, that in England the analogy between the fossils of the Magnesian and Mountain Limestone obtains to a far greater extent than could be supposed from any published catalogues. I trust, therefore, that the ensuing year will not be without its fruits in the production of new works on the shells of the Magnesian Limestone of our own country; and I am glad to have it in my power to inform you, that Mr. King, the Curator of the Natural History Society of Newcastle-on-Tyne, is preparing some excellent materials for this purpose.

A better acquaintance with the Permian fossils, particularly the prevalent Mollusca, induces me, notwithstanding the arguments I employed last year, to infer that this deposit, so naturally connected through its characteristic fossils with the Carboniferous strata, must be classed with the Palæozoic rocks*. The physical structure of Russia is also greatly in favour of this view; for, in large portions of that country, there is an entire absence of the great rupture between

* My companions, M. de Verneuil and Count Keyserling, have long entertained the same views as Mr. Phillips on this point.

the Carboniferous rocks and the Magnesian Limestone, which is so prevalent in the British Isles. The examination of rocks of this age in North America, to which I shall hereafter advert, leads to the same opinion; viz., that the Permian deposits must be viewed as the fourth or uppermost stage of the Palæozoic series, notwithstanding the occurrence of Thecodont Saurians.

Jurassic, Cretaceous and Tertiary Strata.—Overlapped as these Permian deposits are, in certain tracts of Russia, by red and white marls and sands, we are not positively prepared to state (in the absence of decisive fossil evidences) whether some of them may not represent the Trias; though the fossiliferous limestone of Monte Bogdo, in the steppe of Astrachan, is probably of this age. With the Jurassic strata, however, which follow, and which occur at intervals from 65° north latitude to the countries south of the Crimea, we made ourselves well acquainted. Should the word *Jurassic* grate upon the ears of Englishmen, it is impossible to deny that this geographical term is much more applicable to the strata in question than our own word "Oolitic," which implies a structure scarcely ever seen in beds of this age in Russia. Whether examined at Moscow or on the Lower Volga, they consist of black shales and ferruginous sands, occasionally containing calcareous cement-stones, and thus they present a general lithological analogy to the Lias; a formation, however, which is not represented in Russia, for the fossils are all referable to the groups extending from the Inferior to the Upper Oolite, and many of them are identical with British species.

I must now pass over the Cretaceous deposits which occupy such broad spaces in Southern Russia, the Lower Tertiary beds, some of which, on the Volga, might almost be mistaken for those of Bognor and the London basin, and also the strata of the Miocene age, which occupy wide tracts in Volhynia and Podolia, merely remarking by the way, how the recent discovery in limestone of an herbivorous Cetacean (to which I shall elsewhere allude) is a very important addition to the fauna of that period.

Superficial Detritus—Ural Mountains, &c.—The discovery of accumulations containing recent shells of Arctic species, considerably to the south of Archangel, was important, as showing that the great northern blocks, which overlie them there, were brought to their present positions during a period which differed remarkably from the one preceding it, and also from that which has followed it, in the very general prevalence of a colder climate over large spaces; thus enabling us very safely to infer, that the great erratics of the North were transported in icebergs, which floated in an arctic sea and occasionally grated along its bottom. But this operation, gigantic as it was, had its well-defined southern limits, as is beautifully proved by the general survey of the Russian Empire; in the southern half of which all such erratics cease, and fine black slime (tchornozem) takes their place. Wherever the recent accumulations of the steppes indicate the desiccation of brackish seas, which like the present Caspian were inhabited by Mollusca requiring a warmer

climate, then is there also a total absence of great boulders. On this point I would further beg to remind you, that in our examination of the Ural Mountains, even up to 60° north latitude, my companions and myself could trace no evidence whatever of boulders having been transported from these mountains to great distances upon their flanks; although the peaks rise to heights of 5000 and 6000 feet above the sea, and the chain extends from north to south through eighteen degrees of latitude. In every portion of the Ural Mountains, and on both their Asiatic and European flanks, the detritus is of a purely local character; and in it are occasionally entombed the bones of the Mammoth, *Rhinoceros tichorrhinus*, and other great extinct quadrupeds, mixed up with gold sand and gravel. Transporting ourselves to the south of Russia, we find the upper portion of the cliffs of the Sea of Azof composed of local detritus and clay equally charged with the same remains.

The general examination of Russia proves in the most emphatic manner, that the central masses of her continent, though exempt from all plutonic agency, have undergone grand but tranquil oscillations which have scarcely at all disturbed the physical outlines of the ancient bottoms of the sea—oscillations which have operated in a similar manner over this vast space from the remote Silurian age, to the close of the period antecedent to the historic æra. This survey has also taught us, that the great Russian continent is surrounded by rocks of igneous origin, the eruptions of which have corrugated and diversified certain portions of the surface at different periods.

On her eastern or Asiatic side it has been the wish of my friends and myself to endeavour to read off in the Ural Mountains the effects of such derangements, and to trace the sedimentary deposits of Russia through the mazes of that band of great disturbance. Having so recently laid before you the outline of our views concerning this chain, and being aware that you will soon be in possession of much additional knowledge respecting it from the pen of the illustrious Humboldt, I will confine myself to a single paragraph, by saying that our chief object was to refer these broken and altered masses to their normal types.

We found this highly metamorphic chain, so rich in metalliferous masses, to consist essentially of Silurian, Devonian and Carboniferous rocks, the fossils of which we traced at intervals, notwithstanding the countless ridges of igneous matter and the highly crystalline structure which has been communicated by its eruption to the contiguous sedimentary strata. A short period only has elapsed, since rocks having quartzose, micaceous and gneissose characters would not have been admitted into the same category with strata containing organic remains; but the theory of metamorphism, founded on patient observation and comparison, has prevailed over ancient doctrines. The sedimentary rocks of the Ural being palæozoic, must, indeed, be viewed as among the most ancient of the metamorphic class. Many other crystalline chains are of much more recent age, as long ago, indeed, shown by M. Leopold Von Buch and other observers. Of the truth of this I will first adduce proofs

from the Caucasian chain which bounds the Russian Empire on the south. The second illustration of metamorphism will be derived from recent researches in the Western Alps.

Caucasian Chain.—We must now estimate the efforts of an observer, who, in common with Agassiz, does such honour to the little canton of Neufchatel. Though the name of M. Dubois de Montpereux has escaped the notice of my predecessors, an outline of his chief labours was laid before the geologists of France, in 1837, by M. Elie de Beaumont. Attention having been latterly much directed towards Russia and the adjacent regions, it becomes my pleasing duty, the additional researches of M. Dubois having been recently published, and having myself largely profited by them, in the construction of a map of that empire, to invite your consideration to his great work.

From its diversified and varied outline, and its early historical records, no region within the reach of Europeans seemed to have a greater claim upon the combined efforts of geologists and historians than the Caucasus, and yet of no country were we more ignorant. For although travellers have, from time to time, passed over it by one great road or another, from the thirteenth to the present century, and though Kupffer has well described the environs of one of its northern peaks Elbruz, and Eichwald has explored the coasts of the Caspian, we had never had a true picture of the physical geography and varied inhabitants, still less of the geological structure of this chain. No one, in short, struggling with the dangers of climate and uncivilized inhabitants, had so threaded these mountains and defiles as to make us familiar with them*. This Herculean task was undertaken by M. Dubois, and most successfully has he executed it, for his work of five volumes is that of a geographer, historian and geologist. The lovers of Homer, Strabo and Pliny will assuredly find in it a mine of classical recollections, a correct identification of ancient sites and vivid sketches of ancient customs, described by Grecian and Roman writers, some of which habits prevail even to this day. Regretting, as we must, that tracts formerly illustrious in song, and many of them still blessed with the richest gifts of nature, should, for the most part, be now tenanted by wild and barbarous tribes, let us the more admire the zeal and energy with which our author, surrounded by privations, has produced so clear a picture of this extraordinary region. Not only does M. Dubois place before us the physical features and the social condition of the various tribes, from the Circassians on the north to the Georgians and Armenians on the south, but he gives us geological sections, maps and descriptions, and thus brings the rocks of these wild and rugged tracts into a clear comparison with known European types.

* Of the scenery, antiquities and costumes of such parts of the Caucasus as he visited, including special illustrations of Ararat and the north of Persia, my lamented friend Sir Robert Ker Porter brought away a rich series of beautiful sketches, a few only of which have been published. Though not a geologist, his faithful pencil conveys an admirable idea of the character of the highly inclined and metamorphic strata.

No country is more entitled to the name of metamorphic than the Caucasus, for M. Dubois proves, that the oldest sedimentary rocks of which it is composed, are scarcely of higher antiquity than the Lias; whilst Ararat, in great part of recent volcanic origin, exhibits on its flanks streams of lava, which must have flowed since the land has assumed its present configuration. So modern is this mountain in the records of the geologist—so venerable as the cradle of the human race.

Let us however cast a rapid glance over some of the chief results of M. Dubois's researches. The central ridges of this mighty chain, formerly supposed to be of primary age, are nothing more than beds of Lias and other strata of the Jurassic age, often so highly altered as to resemble ancient slaty rocks, and occasionally pierced by points of granite and greenstone. These grand and strangely changed strata of the Oolitic series are flanked by huge buttresses of the Cretaceous system, some members of which take the form of fucoid schists and greensand, whilst others are pure white chalk. Often, indeed, assuming ancient lithological aspects, and broken into striking defiles, covered by beauteous forests, the far-famed Circassia, from one end to the other, is simply the representative, on a grand scale, of our English North and South Downs, with their fringes of greensands*. The explanation of the violent disturbances to which these cretaceous rocks have been subjected, is seen in numerous points of porphyry and other igneous rocks; by which agents, the strata, altered and thrown into highly inclined positions, have been raised to heights of 10,000 feet above the sea. On each side of the flanks of these gigantic mountains, essentially if not entirely composed of the younger secondary rocks, are tertiary basins. To the north they range into the desiccated Caspians, which form the southern steppes of Russia; and to the south they lie inclosed among numerous ridges of plutonic and igneous rocks. Volcanic agency, of the same extinct nature as that with which we are acquainted in Central France, and with which English geologists are now familiar in Asia Minor, from the descriptions of our associates Hamilton and Strickland, abounds indeed throughout many parts of this region. Of the different phases of eruption, the porphyry cone, the crater and the coulée are the evidences; whilst the continued existence of the latent and repressed fires of antiquity is traceable in the naphtha springs of the adjacent lower countries, which are still in action, upon lines running from north-west to south-east, or parallel to the grand line of eruption on which the Caucasus has been upheaved. M. Dubois distinctly marks the great period of elevation of these tracts, when the ridges on the south of the chain began to be the centres of many distinct volcanic vents; and when, by the higher and lesser elevations of districts formerly submarine, the various "amphitheatres" in Georgia and Armenia were formed, all of which he has personally visited. This is truly

* The botanist will find a striking analogy between the vegetation of the western flanks of the Caucasus and the North Downs (Box Hill) in their respective groves of *Buxus*.

the region in which, if it be possible, passages may be traced, from rocks of plutonic or submarine igneous agency, to those of pure volcanic and subaërial operation. To decipher order throughout a tumultuous sea of rocks, bristling with extinct volcanoes, is no small effort; and if M. Dubois should not have completely succeeded in neatly separating the porphyritic and later plutonic eruptions from those of true volcanic age, he has at all events offered proofs, that in many instances the former were covered by the most ancient tertiary accumulations (*quasi* uppermost-secondary), in which vast accumulations of rock-salt, almost mingled with lava of subsequent eruption, seem to favour this hypothesis, that the accumulation of this mineral may in some instances have been connected with igneous agency.

In describing the great elevation which converted the Caucasus from an island into an isthmus, and desiccated large portions of the adjacent seas, leaving them in the condition of steppes, M. Dubois attaches great weight to the evidences of upheaval of the masses in crateriform shapes; and in tracing the succession of the tertiary strata, he shows, that, as a great depression (Colchis) formerly existed between the Caucasus and Armenia, so the beds of rivers which flowed into this ancient gulf, graduate into and form the lowest part of such tertiary basins.

So attractive are his descriptions, that we can actually bring before our mind's eye each successive mutation; either when great and irregular elevations raised up the ancient sea-beds to different levels, or when volcanoes bursting forth (some submarine, and others under the atmosphere) barred up these basins, forming brackish and salt lakes, many of which have since been desiccated. Seeing that he first establishes all the fundamental points of his work on sound observation, and identifies each formation by organic remains, a geologist even may revel with M. Dubois, when, after speaking of the superb garland of volcanic cones, whose summits, ranging from 12,000 to 17,000 feet above the sea, surround the great desiccated basin of Central Armenia, he allows himself to speculate on the letting off of former inland seas and lakes, by the waters of which our progenitors may have been destroyed.

Receding, however, from these views, which connect our science with the history of man, I specially beg to notice the very clear order of the Cretaceous system, first pointed out by M. Dubois on the southern shores of the Crimea; a tract very analogous to the region of the Caucasus, of which, in fact, it is a prolongation. Of the trachytes, trap, pumice, lava and scoriæ of the southern flank of the Caucasus itself, we have no traces in its miniature the Crimea; but a perfect epitome of all the succession of its northern and Circassian slopes, showing a Jurassic series supporting a very complete Cretaceous system, which the author places in perfect parallel with the beds of similar age in Europe, with which he is acquainted.

In referring you to his table which marks twelve distinct stages in this cretaceous system, the uppermost of which contains *Trigonia* and *Ostrea gigantea*, and the lowest of which is an un-

doubted type of what foreign geologists call the *Terrain Néocomien*, I would here suggest that the greater part, if not the whole, of the series may be paralleled in the south of England, and that too without consulting other sections than those described by Dr. Fitton, and the natural phænomena which he has so faithfully represented. The uppermost seven divisions of the Crimea are all evidently referrible to our chalk and chalk marl, and contain many of our well-known fossils, the only striking difference consisting in the association of Nummulites and *Cerithium giganteum*, with the *Trigoniæ* and *Ostreæ* of the uppermost chalk. The beds 8 and 9, *Craie chloritée* and *Grès vert*, containing, amid *Exogyræ* and some cretaceous fossils, the *Pecten quinque-costatus* and *P. orbicularis*, most unequivocally represent our Upper Greensand or "malm rock." The marly strata which lie beneath such beds are therefore, we may fairly presume, the representatives of the English gault; and, lastly, the yellow limestone and sand, immediately beneath it, which forms the Neocomian of M. Dubois, may, after all, be considered the equivalent of our Lower Greensand, or the expansion of its lowest beds which pass into the Wealden.

I have always regretted that so few foreign geologists have obtained an adequate idea of the dimensions and importance of the third or inferior division of lower greensand as exhibited on the southern shores of the Isle of Wight (Atherfield Rocks). In France the formation has been little recognized beyond the Boullonnais, where it has already almost lost its distinctive characters, and where there are no longer the divisions of upper, middle and lower beds, each, as shown by Dr. Fitton, characterized by peculiar fossils. In the south and east of France, where upper greensand and gault abound, it now appears* that the base of the Cretaceous system is composed of limestones identical with those of Neufchatel, and which, participating in all the flexures of the chalk, are usually broken off, as in the Crimea, from the Jurassic system.

It has been suggested that the Neocomian limestones may represent the Wealden of British geologists. But this is not, so far as I can judge, safe reasoning; for the latest researches of Professor Owen would lead me to believe, that the Saurians of that great estuary formation are much more nearly allied to those of the Oolitic or Jurassic epoch than to those of the Cretaceous period; and Agassiz has assured us that the fishes of the Wealden are entirely distinct from those of the chalk.

I have before expressed my opinion on the head of Neocomian, both to French and English geologists†, and I now repeat it, more however as a stimulus to those who have the means to settle this point accurately, and not because I entertain any objection to the foreign use of the word. The term may, indeed, be very well and

* See discourse of last year. [Phil. Mag. S. 3. vol. xx. p. 541.]

† In referring to an opinion which I expressed at the meeting of the Geological Society of France in 1839 (Bull. Soc. Geol. tom. x. p. 392 *et seq.*), I beg to say, that it is specially to the Greensand as the chief equivalent to which I refer.

appositely used in reference to the deposit throughout central and eastern Europe, where its lithological characters are so different from what is I presume the English type; and where they have been so well described by Swiss and French observers. I seek merely for the establishment of the truth; and I again ask if the Neocomian of Neufchatel and the Crimea be not the equivalent of the lowest Greensand of England, and of the *hils-thon* of Römer in Hanover? On revisiting the Isle of Wight last spring, in company with my friend Count Keyserling, and on finding in these beds many true Neocomian species, I adhered to my old opinion*; and I now put this question in the hope that it will be completely answered through the labours of English geologists, and particularly of Mr. Austen, who has, I know, commenced an inquiry into this subject, and whose acquaintance with fossil shells and habits of field-research well qualify him for such a task.

There is yet one point connected with the researches of M. Dubois on which I beg to touch, from the admiration I entertain for any one, who pursues science for its own sake, and achieves durable results by his own unassisted endeavours. Occupied during ten years of his life as an instructor of youth, M. Dubois had no sooner realized a small independence than he resolved to enter upon this arduous undertaking. Repelled, in the first instance, by the war with Turkey and by the plague, he fortunately retired upon Berlin, where, passing two years in the admirable school of geologists of which that capital boasts, he once more set forth on his grand enterprize, with no other recommendation than that which his good name, and a short memoir on Volhynia and Podolia, had acquired for him, and no other means than his own very moderate private fortune. Disposed, as it has always shown itself to encourage science, the Russian government was no sooner acquainted with his designs, than it offered him conveyances in their ships of war, and subsequently gave other encouragement. Hence M. Dubois was enabled to reach parts of Circassia which would otherwise have been inaccessible; and thus he entered upon his remarkable journey. Revisiting the Crimea and parts of the south and north of Russia, he returned to Berlin, after an absence of four years, laden with much precious knowledge. But how was he to put this before the world? Not alarmed at the prospect of publications, from their descriptive nature necessarily very expensive, M. Dubois, encouraged by M. de Buch and M. E. de Beaumont, commenced the preparation of his works, of which five volumes and a splendid atlas have already been issued; and as these are to be followed by other works, it is to be hoped that all the productions of this spirited author will be adequately and liberally purchased by the discerning portion of the public. I have the less hesitation in

* Since this Address was read I have received a letter from Count Keyserling, dated Petersburgh, March 7, in which he acquaints me, that in a mass of shelly rock from Kyslavodsk in the Caucasus, which has been considered Neocomian, he has detected the same species of *Thetis*, *Trigonia*, and other fossils as those which we collected together in the Lower Greensand of the Isle of Wight.—*March* 31*st*.

making this appeal to my countrymen, because I really believe that the high class of merit which belongs to the researches of M. Dubois is yet known to very few of them*.

Under the head of *Asia Minor* Mr. Murchison refers to the geological researches of one of the Secretaries of the Society, Mr. W. J. Hamilton, as having connected the region described by M. Dubois with the southern Mediterranean types; notices, under the head of *Turkey in Europe, Servia, &c.*, the investigations of M. Boué, uniting the distant regions of Asia Minor with Western Europe; and he then briefly relates some of the prominent results of the researches of M. Sismonda in the *Piedmontese Alps*. He next proceeds to "North American Geology," in which he alludes to the communications on that subject contained in Silliman's American Journal of Science; also to the Memoirs of Professor W. R. and D. H. Rogers, and to the papers and views of Mr. R. C. Taylor, Mr. Featherstonhaugh, Dr. Dale Owen and others; he then proceeds to the

Theory of the Origin of Coal.—American and European Evidences compared.—At the last Anniversary we were aware, from the independent evidence of Mr. Lyell, that both the bituminous and anthracitic coals of Pennsylvania were underlaid by *Stigmaria ficoides* and fireclay; and we have now before us the result of the labours of our associate Mr. Logan in the coal-fields of Pennsylvania and Nova Scotia, in examining which his chief object seems to have been to ascertain whether the facts relating to the theory of the origin of coal, as seen in North America, were analogous to those to which he has so successfully directed attention in England.

Availing himself of the prior researches of the American geologist, Professor H. Rogers and his assistant surveyors, who had prepared the valuable map of Pennsylvania above alluded to, Mr. Logan has laid before us a very clear sketch of the general relations of the Pennsylvanian carbonaceous deposits, and of their chief convolutions. Since that time the Governor and legislature of the Canadas have wisely selected this well-trained field geologist to execute a mineral survey of the whole province; and I am happy to acquaint you that he has already commenced his task in a very effective and vigorous manner, by laying down as the base-lines of his work some of the great anticlinals and synclinals of that region, and by connecting them with the already described feature of the United States. In comparing the coal-field of Pennsylvania with those of South Wales, with which he is familiar, Mr. Logan states, that he almost invariably detected beneath each anthracitic coal-seam a bed of fireclay or argillaceous materials filled with *Stigmaria ficoides*. In his description of the coal-fields of Nova Scotia, which have not yet been fully developed, but among which we hear of one bed of clear coal twenty-four feet thick, and affording 250 tons daily, Mr. Logan states he had also detected the *Stigmaria ficoides* in similar underclay. With such extended observation spread out before them, the evidences in which all seem to point one way, young geo-

* For the general geological views of M. Dubois, see his letters to M. E. de Beaumont, 'Bulletin de la Soc. Geol. de France,' vol. viii. p. 371 *et seq.* His researches have been justly appreciated in France.

logists may well be led to suppose that the theory which, if I may so speak, has recently been rendered fashionable, of the origin of coal by subsidence of vegetable matter *in situ*, must be considered established as of general application. I, however, adhere to the cautionary remarks which I ventured to make last year, and will now endeavour to impress upon your minds the inapplicability of such a theory, however true under limitations, to large portions of the carboniferous strata in different parts of the world.

Since our last Anniversary statements have appeared in our own country, both supporting and impugning the probable truth of the theory. The last meeting of the British Association being held at Manchester, geologists were there assembled in the centre of a tract appealed to with great reason by the supporters of this theory as containing many proofs of its truth; for, in the immediate vicinity of that town there occur, as you all know, the beautiful examples of vertical stems of large trees apparently in their original position, which were formerly described before this Society. After giving an elaborate and satisfactory account of the great Lancashire coal-field, showing that its lowest members, formed on the flanks of the Penine chain, and subordinate to the millstone grit, contain marine shells analogous to those of the Mountain Limestone series, and stating that they are surmounted by a middle and an upper group, the former constituting the richest coal-field, Mr. Binney describes in great detail the composition and contents of all the numerous roofs and floors, as well as also of the coal-seams, which are included between them. He shows also that the roofs vary in their nature at different places, even over the same seam, and contain the remains of many vegetables, sometimes, as near Manchester, in vertical positions, *Sigillariæ* being in such cases a most abundant plant; other roofs of black shale in the lower field are loaded with *Pectens*, *Goniatites*, *Posidonia*, and fishes. The coal-floors, on the contrary, present a much greater uniformity of structure, fireclay similar to the underclay of Mr. Logan being most abundant; though it is admitted, that a different or siliceous clay also frequently occurs, and that two instances are known where the coal rests at once on coarse quartzose sandstone. Seeing, that with one exception, all the floors throughout an estimated thickness of near 5000 feet contain the plant *Stigmaria ficoides* usually with its leaves attached,—that both the roofs and floors indicate a very tranquil method of accumulation,—that the coal is free from admixture of foreign or drifted materials, and that large trees frequently stand upright, this author is induced to believe that the vegetables out of which the coal has been formed, grew upon the spot.

At the same meeting this view was contested by Mr. W. C. Williamson, also well acquainted with the structure of the country around Manchester. His chief arguments were, however, derived from other tracts, and they assisted in proving,—1st, the frequent association of marine shells with coal (as at Coalbrook Dale, and in Yorkshire); 2ndly, the very triturated and broken condition of the plants, as well as their great intermixture in the sandstone and grits, coupled

with the fact that large quantities of vegetables are often matted together with marine and estuary shells, phænomena indicative of drift. Admitting that the floors of the coal or underclay present a great uniformity both in the absence of other plants and in the almost general occurrence of the Stigmaria, Mr. Williamson allows that a plant, found so very generally in such a position, may have grown in estuaries into which the other vegetables were drifted. Acknowledging that the drift theory is open to some objections, he stated that one of the greatest of these is, in his opinion, the extent and uniformity of some of the thin seams of coal. On this point, however, I must be permitted to say, that, if admitted, the difficulty must be applied to numberless other deposits of all ages, which every one knows must have been accumulated under water. Subaqueous action of a tranquil nature is, it appears to me, precisely the agency by which we can satisfactorily explain the uniformity of many thin layers containing vegetables which are extended over wide areas, as in the copper grits of Russia before alluded to. By what other possible means, for example, can we explain the wide extent of the thin copper slate of Germany with its associated fishes on the still thinner bone-bed at the base of the Lias? So far then from being a phænomenon which invalidates the formation of coal under water, it seems to me, that the very fact of a thin and equable deposit is an almost impossible condition, if we insist exclusively upon the submergence of forests or jungles *in situ*, in which considerable irregularities of outline must in all probability have prevailed.

On my own part, and that of my fellow-travellers in Russia, I have brought before this Society what we consider strong evidences against the too general adoption of this favourite theory. We have told you that in many instances the *Stigmaria ficoides* occurs in loose and incoherent sands, as well as in shales, and is frequently present where no coal is seen ; but what we chiefly insist upon is, that all the coal-seams of the South of Russia, without exception, alternate repeatedly with beds of purely marine origin. In one section of the Donetz coal-field it has been stated, that at least twelve beds of marine limestone alternate in one vertical section with thirteen seams of coal and numerous bands of sandstone and shale, in which many species of plants, besides Stigmariæ, are confusedly heaped together. But we need not go to Russia for such examples. The whole of the mountain limestone or lower coal series of the north of England is charged, though not to so great an extent, with proofs of the alternation of marine deposits with coal and its associated sandstone and shale.

The coast of Northumberland, to the north of Alnwick, presents evidences of thin seams of coal resting at once on sandstone, and intimately connected with limestone full of sea shells. Advancing northwards to Berwick, and to beyond the Tweed, purely marine strata re-occur, charged with still more carbonaceous matter ; and, in the same series on the north-western parts of England, we have frequent examples of the persistence of what must be called exclusively marine conditions. Throughout that vast succession of

beds, all the animal remains with which geologists have become acquainted, occupying many distinct stages, have lived in the sea, whilst the plants, so far as I have been able to observe them (broken into fragments), consist of many species irregularly heaped together, the whole, together with the sands, grits, pebbles and shale, offering the clearest signs of the drifting action of water.

On the subject, then, of the origin of coal, it would appear, that as our inductions can never be sound, if they repose upon one class of phænomena only, so do some coal strata offer indications of the truth of the hypothesis, that in large tracts of the world, the mineral was formed from vegetables which were washed into bays and estuaries, and often carried far into the then existing seas. In other instances, flat and marshy tracts rich in tropical vegetation, being subjected to gradual depressions, may have been converted into lagoons and swamps without any direct encroachment of the sea; and in this peculiar condition (subjected, however, in all cases, to entombment beneath those waters in which the overlying sandstone and shales were accumulated), oscillations of the land may have raised the beds at intervals, again to be fitted for the growth of marshy vegetables.

In geology more than any other science, it must be our constant endeavour to unravel phænomena which at one time seemed inexplicable, and often opposed to each other; but with new discoveries the difficulties vanish, and the apparently conflicting testimonies are found to be in perfect harmony with the order of changes, which the surface of the globe has undergone. I repeat, therefore, my belief, that, whilst coal may have been formed in many localities by subsidence of vegetables on the spot on which they grew, as first suggested by Brongniart, MacCulloch and others, its origin unquestionably is also due, and over very large territories, to plants having been washed into estuaries and seas, and there equally spread out in successive layers with sand and mud.

Gypsiferous Rocks in North America.—Having now disposed of all the subjects relating to the known deposits of decided Palæozoic age in North America, I will endeavour to show how the examination of one continent throws light upon the structure of another, by inviting your attention to the great Gypsiferous deposits of North America, to which, in treating of Russia, I have already alluded.

The gypsiferous strata of Nova Scotia, with their associated sandstone, shale, and fossiliferous limestones, were at first referred by Mr. Logan to the triassic period; an inference which he drew from the general character of the fossils, and their dissimilarity, as a whole, to those of the Carboniferous rocks of that country. This opinion, however, is one from which I know this author receded, upon finding that some of the shells which he had brought home were recognised by M. de Verneuil and Count Keyserling, to be identical with species from the Permian deposits of Russia.

This comparison with the Russian strata has, indeed, received so much illustration by the arrival of a large assemblage of fossils

brought from numerous localities in Nova Scotia by Mr. Lyell, and which he has obligingly submitted to the examination of M. de Verneuil and myself, that I have not much doubt of these gypsiferous deposits and associated limestones of Nova Scotia, being absolutely the same as the Permian rocks of Russia. Even lithologically there is the greatest similarity, whether in the large rock-masses of gypsum, red and green marls, conglomerates and sandstones, or in the magnesian and sandy limestones; and when we compare the fossils submitted to us, the parallelism is as firmly established as can be, between any two groups of the same age in distant localities. It is not merely that these American strata contain a few species identical with forms which typify the Magnesian Limestone of England, the Zechstein of Germany and the Permian rocks of Russia, but still more that such beds immediately overlying the carboniferous deposits, and even, according to Mr. Lyell, partaking of some of their flexures, should be found to contain exactly the same distribution of genera, and as near as possible the same proportions of species in each genus as in the synchronous deposits of Europe! Again, the fossils of this group are, for the most part, as badly preserved and limited in species in America as in Europe, and the striking agreement of those which can be detected is therefore the more remarkable. Even in negative proofs the similitude is great; for wherever these deposits have been traced eastward through Europe and to the confines of Asia, they have been found to be singularly deficient in chambered shells, and such Mr. Lyell finds to be the case in the various localities examined by him in North America. But having already sufficiently called your attention to the striking points of agreement between the American and Russian formations, I anticipate the pleasure you will shortly experience when Mr. Lyell brings the subject, as he intends to do, before the Society.

Seeing the great variety of lithological aspects of these strata in Russia, and that the flora as well as the fauna are of a type distinct from those of the carboniferous age, we proposed the name *Permian*, a term which I trust may be considered more applicable to the equally diversified deposits of North America than "Zechstein" or "Magnesian Limestone,"—names which point to one member only of this complex series as seen in Russia, and where it occupies a region larger than the whole kingdom of France!

Mr. Logan having also stated that he found slabs in some rocks in the bay of Fundy, which he considers to be of the same age, and which exhibit footmarks on their surface, is it possible, I would ask, to connect with the same formation, the red and green marls of the valley of Connecticut, though distant 400 miles, in which Ornithichnites occur, and which also contain remains of Palæoniscus, a genus of fish very characteristic of the Permian rocks? To this question we shall again revert under the head of Palæontology, in considering the Ornithichnites of Connecticut*.

* Since the Anniversary of the Society the fossils of the gypsiferous rocks of Nova Scotia collected by Mr. Logan have been examined by Mr. Phillips,

Newfoundland.—This very ancient British colony, viewed until recently as a mere fishing station, but now rising rapidly into importance through its internal sources, has recently undergone a geological survey by one of our members, which demands notice in this portion of my Address, because the author, Mr. Jukes, is of opinion, that this island contains no strata of younger age than the Carboniferous. The eastern parts are, it appears, composed of very thick deposits of slaty rocks, sandstones and conglomerates, which are divided into upper and lower masses. They are penetrated by different igneous rocks traced from north-north-east to south-south-west in a number of anticlinal and synclinal lines. Great masses of the central tract are usurped by granite and various igneous with metamorphic rocks, which are followed on the west by a band of gneiss and mica schist with crystalline limestone*. To what epochs any of the slaty rocks belong, has not been determined, as no organic remains have been found in them, but on the western shores, this ancient and crystallized series is overlapped by red sandstone, shale, gypsum, beds of coal subordinate to shale, marl, yellow sandstone and grit. I here quote the ascending order which Mr. Jukes assigns to the strata; but as he admits he never could observe consecutive sections, is it not possible that the great gypsiferous beds of Newfoundland may occupy the same place in relation to the coal-fields as in the opposite shores of Nova Scotia, and like them represent the Permian deposits? In fact, Mr. Jukes candidly states, that the gypsiferous, red and inferior portion of the coal formation (as he classes it) is so similar to the New Red Sandstone of England, that he was at first sight tempted to give it that name. Now as these rocks are seen in one section only beneath the coal, the following hypothesis may be adopted: that the coal in question is not a portion of the great old coal formation, but of the same age as the coal in the Permian rocks of Russia, and that the strata have been inverted where our author examined them, a phænomenon easily understood in a region so highly metamorphosed as Newfoundland, and where rocks of the age of the Magnesian Limestone may have been locally placed beneath true carboniferous strata. It would be wrong, however, to attempt more than mere suggestions from any evidence which has yet been brought before us, since Mr. Jukes has found no traces of organic remains even in these uppermost deposits of Newfoundland. In truth, our associate has evidently had to grapple with some of the most ambiguous rock-masses of North America, in a country obscured by moss and vegetation, as yet

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who is of opinion, that they bear a most striking analogy to those of the Magnesian Limestone of England. It is satisfactory, therefore, to know that the beds containing the footmarks are proved to be of the same age with the gypsiferous rocks by the presence of the same group of fossils. Mr. Logan alludes to plants which I have not seen, and the exact comparison of these and others collected by Mr. Lyell with Permian types is still very desirable.—April 1st, 1843.

* See also some very interesting observations on the structure of Newfoundland in Sir R. Bonnycastle's 'Newfoundland in 1842,' vol. i. p. 179.

impassable to the casual traveller, and the coasts of which are of very difficult access. He deserves, therefore, so much the more our thanks for having pioneered the way under many difficulties, and for giving us this outline reconnaissance of the geological structure of a colony, to become well acquainted with which will require elaborate surveys, conducted by those who have previously made themselves masters of the keystones of succession in the adjacent continent. When, therefore, the true geological equivalents of Canada and Nova Scotia shall have been thoroughly established by the researches of Mr. Logan, and placed in exact relation to the well-developed rocks of the United States, the obscurity which shrouds Newfoundland may be dispelled*.

Secondary and Tertiary Rocks, and superficial deposits of North America.—In my Address of last year I had no hesitation in predicting, that geologists would reap great instruction from the visit of Mr. Lyell to the United States. The earlier sketches which he sent to us, including accounts of the Palæozoic rocks, might be taken, indeed, as some earnest of what was to follow, and as we are well acquainted with his powers of generalizing and habits of faithful research, we could not well over-estimate the amount of production at his hands. The documents which he has laid before you have fully justified our anticipations. One of his memoirs, on the Tertiary formations and their connexion with the chalk in Virginia, North and South Carolina, and other parts of the United States, has a very important bearing in showing the amount of agreement of those deposits with the strata of similar age in Europe. Noticing with due approbation the works of Professors W. B. and H. D. Rogers and Mr. Conrad, on the Tertiary rocks of Virginia, he shows, that certain deposits above the chalk are of true Eocene character, and never contain Secondary fossils or any forms intermediate between the newer Secondary and older Tertiary types. These Eocene beds are surmounted by rich shelly deposits, the contents of which bear a great generic resemblance to those of the Suffolk crag and the Faluns of Touraine, and are therefore referable to the Miocene epoch.

In North Carolina, black shales, first described by Mr. Hodge, are shown to be of the cretaceous age by containing Belemnites, Exogyræ, Gryphææ and Ostraeæ, a few of the species being well known in Europe, and found by myself in the distant parts of Russia. This cretaceous deposit is covered by a peculiar calcareous rock, the Wilmington limestone and conglomerate, which had been termed Upper Secondary, and supposed to indicate a passage from the Secondary to the Tertiary periods, but in which Mr. Lyell could detect no organic remains to support that opinion, the only

* I regret that I accidentally omitted to call attention in my last Address to a short memoir, read during the preceding session by Mr. Henwood, upon the Silurian Rocks of Lockport near Niagara. Having long been assiduously occupied in his native county Cornwall in studying the mineralization of rocks, Mr. Henwood is, I understand, about to publish a work on the metalliferous deposits of Cornwall and Devon.

determinable species being of the Eocene age. Again, in South Carolina, on the Santee river, a white limestone occurs, which lithologically so resembles one of the upper members of the cretaceous deposits of New Jersey, that even Mr. Lyell, at a first view, had no doubt it was a portion of the same formation: on examination, however, of the fossils, it proved also to belong to the tertiary series. This lithological resemblance had erroneously led to the admission of several well-known tertiary fossils into the Cretaceous system of America, an error which Mr. Lyell has removed. This correction is valuable, and, though it tends to negative a hope which I once entertained, founded upon what my friend Professor Sedgwick and myself believed to be very good evidence on the flanks of the Austrian Alps, that beds of passage would be discovered between the Cretaceous and Eocene epochs, I am bound to say that the transatlantic researches of Mr. Lyell go far towards the establishment of an extensive, though I still incline to consider not a general break between those periods; for he prudently admits, that evidences differing from those he obtained, may be found in the Southern states bordering the Atlantic, of which he explored but a small part. Referring you to the abstracts of his memoirs, and knowing that you will soon have from him more complete details, I will not occupy your time in attempting to give what would convey an imperfect idea of the succession of the widely spread tertiary deposits, which occupy nearly all the portion of Georgia and South Carolina between the mountains and the Atlantic. Illustrating the observation of Mr. Maclure, that the first falls of the Savannah and other rivers of this region are at the junction of the tertiary strata with granitic and hypogene rocks, Mr. Lyell shows, that at some points, as near Augusta in Georgia, where the former have been made up of the detritus of the primary rock, they have the aspect of gneiss; a fact quite analogous to that which I had the pleasure of observing in his company many years ago in Central France, where the oldest tertiary and freshwater beds repose at once upon the granites of the Puy en Velay. After a laborious comparison of a profusion of fossil shells from the American strata, in the determination of which he acknowledgés the liberal assistance and co-operation of Mr. Conrad, Mr. Lyell sees fresh and strong grounds for adhering to his former views respecting the value of testing the age of tertiary strata, by the smaller or greater per-centage of existing species which are to be detected in each deposit; for he finds the same proportions which had been established between the fossils of European basins and the living mollusca of adjacent bays, to hold good in the Eocene and Miocene deposits of the United States, when compared with the existing fauna on their shores.

In supplying us with new evidences of the recession of the Falls of Niagara, which he described last year, Mr. Lyell has also given us a sketch of the ridges, elevated beaches, inland cliffs and boulder formations of the Canadian lakes and valley of St. Lawrence. After referring to the researches of Capt. Bayfield at and around Montreal and Quebec, he enters upon a general survey of the great boulder

formations on the borders of the Lakes Erie and Ontario; and states that in the valley of the St. Lawrence, as far down as Quebec, marine shells of arctic character have been found associated with coarse detritus. As some of this shelly and boulder deposit lies at about 500 feet above the sea, and as Lake Ontario is at a much lower level, it is inferred that the sea in which the drift was formed extended far over the territory bordering that lake. That the same sea extended as far south as 44°, 30′ north latitude, is proved by the presence on the shores of Lake Champlain, of marine shells; which, in their Arctic forms and close agreement with those of Uddevalla in Sweden, formerly described by himself, are supposed to imply, like those of the St. Lawrence, the former prevalence of a cold climate when the drift originated. In regard to the far transported boulders, they have in one locality (Beauport) been found both above and below the sea-shells.

The parallel and continuous ridges of sand and gravel, which by Mr. Roy and other authors had been considered to be the shores of an enormous lake, successively let off, are said to rest on clay of the boulder formation, and yet to be occasionally capped by blocks of granite and other hard rocks. Comparing them with the *Osars* of Sweden, and stating, from the evidence of Mr. Whittlesey, that their base-lines are not so horizontal as had been supposed, Mr. Lyell inclines to the belief, though no shells have been found in them, that they were all formed under water, and probably beneath the sea, as banks or bars of sand, admitting at the same time that some of the less elevated ridges may be of lacustrine origin.

The last observation seems to open out the whole question of whether vast freshwater lakes, extending far beyond the area of those which now exist, may not, at one period, have covered the interior of America.

This opinion has been long entertained by our associate, Mr. Featherstonhaugh, who, in his researches eight years ago, amid the western and untravelled tracts, where the sources of the great rivers are separated from each other by very slight elevations, discovered fluviatile and lacustrine shells, wherever excavations existed or pits had been sunk, and at great distances from the courses of the present streams. I have the more pleasure in making this allusion to the geological labours of Mr. Featherstonhaugh, because he near fifteen years ago pointed out some of the chief phænomena connected with the retrocession of the Falls of Niagara. He was among the first persons, subsequent to his survey of large tracts of the far-west country of Arkansas, to assist in the introduction into the United States of an acquaintance with the most modern school of English geology; and who, after popularizing the subject by public addresses in 1828 and 1829, urged upon the government of that country that geologists should always accompany geographical surveyors *.

The view adopted by Mr. Roy, Mr. Featherstonhaugh and others, of the former presence of inland lakes in North America

* See Monthly American Journal of Geology, 1831, by Mr. Featherstonhaugh.

larger than those which now prevail, has recently been sustained by the Rev. Mr. Schoolcraft, an American geographer, in a memoir which he read before the Geological Section of the British Association at Manchester. This author, who has passed nearly twenty years of his life in their vicinity, believes that the former great lakes have been lowered by ancient dislocations. As examples of the bottoms and edges of these former sheets of water, he adduces large belts and tracts of sandy plains, which, from their scanty vegetation and undulated surfaces, have all the appearance of recent desiccation; and as proofs of the water having stood at various levels, he states, that it has left marks of erosion on the mural faces of the harder rocks. But the most original part of this communication, and which may indeed serve to explain the origin of some of the ridges respecting whose origin Mr. Lyell differs from the writers before alluded to, is the actual production of sand-storms by causes associated with these lakes. Indicating some of the most extensive energies of this nature proceeding from Lake Superior, and the powerful action of storms upon sandstone and grauwacke rocks, Mr. Schoolcraft is of opinion, that by a union of powerful currents and furious gales, dunes have been formed which rise to 300 feet above the water. The sand, being first worked up in great bars, has since been transported by the wind over wide tracts, which are thus rendered sterile; stagnant pools are formed in adjacent depressions, once highly productive, and prostrated and buried trees are there associated with freshwater shells; and thus by actual causes, formations of considerable thickness are accumulated. Geologists have long been aware, that wind has been an agent in heaping up some of the deposits whose origin they endeavour to explain, and very striking examples of this operation were adduced by Lieut. (now Capt.) Nelson, R.E., in his account of the modern shelly and sandy limestone of the Bermudas. As no one, indeed, has a better acquaintance with this class of phænomena than Mr. Lyell, it is enough for me to have attracted his notice to the vivid descriptions of Mr. Schoolcraft, which may, I think, aid in explaining some of the superficial appearances in the lake country of North America.

Let us return, however, to the memoirs of Mr. Lyell. Reviewing the series of changes which have taken place in the Canadian and Lake region, Mr. Lyell conceives, that after an early period of emergence, during which lines of escarpment and valleys of denudation were excavated in solid rocks, the surface of the country was submerged, and the cavities filled with the marine boulder formation; and that during the last elevation of the land, the parallel sand ridges were produced, the boulder formation partially denuded, and the different lakes probably formed in succession, leaving a partial sea channel, which, contracting first into an estuary, was eventually converted into the river Niagara. Reaching this point in his order of events, our author succeeds most happily in developing his views concerning the retrocession of the falls of this river; bringing forward arguments to show that during the re-emergence of the land from the sea, a succession of falls must first have been established

near Queenstown. The first or uppermost fall, he argues, must have been of moderate height, when the land was sufficiently raised to wear away the Niagara shale, and undermine the incumbent limestone, which is of slight thickness at its termination near Queenstown. This upper fall having thus cut its way backwards, while the remainder of the escarpment was still protected from denudation by submergence, the second fall would next display itself on a further upheaval of the land, the river being thrown over a lower ledge of hard limestone ; finally the land continuing to rise, a third cataract would be caused over the hard quartzose sandstone, which rests on the soft red marl at Lewistown. These several falls would, at first, each recede farther back than the one immediately below it, their distance being greater or less in proportion to the slow or rapid rate at which the land emerged, but they would all at length be united into one fall, the uppermost limestone becoming thicker in its prolongation up the river, and thus retarding the retrogression of the highest cataract, while the two lower falls would continue to recede at an undiminished pace, until each had in its turn overtaken the uppermost.

In describing the coast sections of Massachussets, and in transferring our attention to the interior of Kentucky—localities already rendered classic by American geologists and palæontologists—Mr. Lyell has placed before us a clear view of other leading points of the changes which that great continent has undergone.

The tertiary deposit at Martha's Vineyard, on the coast of Massachussets, had, indeed, been described by Professor Hitchcock, who seeing the highly-inclined position of the beds, the great variety of structure as well as colour of the strata, and the obscure casts of shells which they contain, was much impressed with their apparent similarity to the Lower Tertiary beds, or Plastic and London clay of the Isle of Wight described by Mr. Webster. By a careful examination however of these strata, and by collecting a larger and more varied suite of organic remains than was known to Professor Hitchcock, Mr. Lyell has come to the conclusion, that so far from being of the Eocene age, this formation is at most of no higher antiquity than the Miocene. This result has been obtained by finding the teeth of several specimens of fishes which belong to species obtained by Mr. Lyell in the Miocene "Faluns" of Touraine, and determined by Agassiz; together with vertebræ, referred by Mr. Owen to two species of whale, the teeth of a seal, and the skull of a walrus; an association which cannot fail to convince geologists that the materials of this island, off the coast of New England, were accumulated at no very distant geological epoch. The high inclination of these party-coloured sands, clay and conglomerates, and the curvatures which some of them have undergone, probably through great lateral pressure, are clear proofs that they have been powerfully upheaved and dislocated; whilst the gravel and boulder formation which covers their edges horizontally, compels us to conclude, that the disturbed beds were submerged during the boulder period, and subsequently elevated to the position in which we now see them.

The presence at Big Bone Lick, on the Ohio, in Kentucky, of great quantities of bones of buffaloes near the spot where salt sources issue through marshy lands, and the existence of the beaten tracks by which these animals approached this spot, render it highly probable that they were allured thither during certain seasons in extraordinary numbers, and that many of them were engulfed and destroyed in the marshy ground.

Mr. Lyell endeavours to show that what occurred within the historic æra to the buffalo, in all probability occurred also to the extinct mammals, whose bones are found in the subjacent clay and marsh to a depth of twenty-five feet, and are associated with modern fluviatile, terrestrial and lacustrine shells, showing that floods of the Ohio have drifted and re-arranged buffalo bones at higher levels than the comparatively ancient marsh. Mr. Lyell suggests, that no great physical revolution of the surface has taken place since the Mastodons died and were buried on the spot, and at a period not very remote from that in which we live. All these remains of extinct quadrupeds, including a horse, which from the incurvated form of its teeth Professor Owen believes to have been of a different species from that which is now living, are said to have existed, as far as the author can judge, all over North America*, at a later period than the deposit of the great boulder drift when the continent was submerged beneath the sea which contained shells of modern species. In connexion with this subject, allusion is made to the observations in South America, of Sir W. Parish and Mr. Darwin; who found that the great Megatherioid quadrupeds lived at a very modern geological period.

The same conclusion respecting the relative age of these fossil quadrupeds and the recent molluscous fauna, is fully substantiated by a clear section and an interesting memoir by Mr. Hamilton Couper of Georgia, recently read before this Society. This author acquaints us, that a shelly post-pliocene deposit, which extends far along the coast, and embraces exclusively marine shells of existing species, is covered by a swampy accumulation, in which the tusks of mammoth and mastodon, often in excellent condition and little abraded, are grouped with the remains of the megatherium, horse, &c. All this indicates a very tranquil deposit, a slow and gradual emersion of the bottom of the sea, and a long-continued elevation of the land during the period of those great mammals which have since passed away and given place to man and the present races.

In bringing before us such a number of clear proofs of successive oscillations of the continent of America, drawn from his own observations and those of other authors, and in generalizing on them with his usual skill, Mr. Lyell further deduces a very important corollary from the Arctic character of the shells in the most recent marine or boulder formation of the northern part of the American continent. For, as there can exist no doubt, that whenever and wherever these shells were deposited, whether at Uddevalla in Swe-

* Has any comparison been yet made between the teeth of the American and the European fossil horses?

den, near Archangel in Russia, in Great Britain or in America as far south even as Lake Champlain, a very cold climate must have prevailed; and as such submarine accumulations were elevated and formed land before the great mammals in question appeared; so it is manifest, as Mr. Lyell remarks, that these creatures could not have been destroyed by the same cold as that which gave rise to the Arctic shells, and with them to the correlative phænomenon of the transport of great boulders and the scratching and scorings of rocks. This clear reasoning appears to me to be an unanswerable refutation of a leading feature of the glacial theory as propounded in its widest sense. At the same time it must be admitted, that the surface of Great Britain does not offer the same neat division between a former submarine state and beds containing the remains of extinct quadrupeds, as the continent of America. In some cases, it is true, as at Market Weighton, described by Mr. W. Vernon Harcourt, there are accumulations of bones which lay in fine shelly clay, with gravel below and gravel above the clay, indicating changes from terrestrial and freshwater to submarine conditions. The Brighton breccia of Dr. Mantell is a fine example of a thick detrital mass, of whose grandeur, and of the powerful agents by which it was heaped up, any one who looks at its composition and at the extraordinary erosion of the surface of the chalk on which it rests, will be convinced; and the bones of elephants are impacted in the very heart of this mass.

Nay, nearly the whole of the cliffs of the eastern shores of England, and large tracts in Norfolk and Holderness in Yorkshire, exhibit, as you know, boulder and detrital accumulations of very tumultuary characters, in which the remains of these great mammals are entombed, sometimes, indeed, mixed up with broken fragments of the same species of shells which in America, it would appear, lie always beneath such bone deposits.

Seeing, therefore, these great differences in the character of the evidence, to what other conclusion can we come, than that the destruction of these great animals commenced at earlier periods in some regions than in others; and that, whilst in America a gradual and steady elevation of the land has preserved records of tracts, which, never since submerged, have been inhabited by successive races of quadrupeds, other countries have been affected from earlier periods by unequal and perhaps more intense oscillations, by which the relations of these animals to submarine and terrestrial conditions have been rendered much more obscure? In a word, the surface of the earth exhibits, in some of its last phases, numberless proofs that no simultaneous general destruction of any such lost races can have taken place; but that each great region, when studied in itself, presents, in the extended sense of the word, local phænomena of accumulation, destruction and renewal.

South America.

This year is marked by a great accession to our acquaintance with South America, by the appearance of the splendidly illus-

trated work of M. Alcide d'Orbigny, published at the expense of the French government, and for which geologists have been long waiting with impatience. During eight years of research, this gifted naturalist successively examined the coasts of Brazil, the Republic of Uruguay, the Argentine Republic from the frontiers of Paraguay to Patagonia, the coasts of Chili, Peru and Bolivia; and by a long residence in the last-named country he was enabled to survey, in many directions, a large region from the coast to the interior. The details of his labours form a first part of the work, illustrated by many sections and by one of most beautifully coloured geological maps which ever fell under my observation. In his general observations M. d'Orbigny remarks, that his own observations extend from 12° to 42° south latitude, and from 45° to 80° longitude west of Paris; a surface comprised between the coast of the Atlantic Ocean in Patagonia to Lima on the Pacific; whilst, by collecting the observations of other travellers and examining fossils from more distant localities, his general views may be said to apply to all the vast continent between Colombia on the north and the Straits of Magellan! This author reviews in chronological order the different rocks, and indicates each change which they have undergone, describing the granitic, porphyritic and trachytic masses in relation to their extension, composition and elevatory agency. He then considers in an ascending order of date the sedimentary deposits, which he classifies as *Gneissic* or Primary, Silurian, Devonian, Triassic, Cretaceous, Tertiary and Diluvial or Detrital; showing the dislocations which they have undergone at successive epochs, and the causes of these disturbances.

After an enumeration of all the facts, M. d'Orbigny, under the head of conclusions, sketches out all the great revolutions of which South America has been the scene; a subject on which he seems to display much vigour of thought, but which I cannot now attempt to analyse, without doing injustice to him, not having, in truth, had time to study his work. One only of his inferences I will advert to, as being clearly established by the order of the evidences; viz. that, as the increment of fresh matter has successively taken place from east to west, so the ancient beds of the sea have been heaved up successively on lines trending from north to south, and to the west of that primary, or original nucleus on the Brazilian shores.

Mr. Murchison next proceeds to the consideration of his subject as relating to "Eastern Countries," inclusive of *Hindostan*, *Affghanistan*, *China* and *Egypt*, terminating the section by the following remarks relative to Egypt and Syria.

After all, it must be allowed that, with the exception of the fossil forest, and the recent elevation of her shores which separated the Mediterranean from the Red Sea, Egypt presents fewer phænomena to interest the geologist than any region of similar range over which researches have extended; for this mass of land seems to have been above the waters during the whole of the ancient periods of

which other regions afford such long registers in the contents of the Palæozoic and succeeding deposits.

We have, however, but to advance northwards to the Lycian Taurus, where Mr. E. Forbes has made known to us the elevation to great heights of tertiary marine shells; or north-eastwards to Syria, where the Dead Sea, as now computed, lies upwards of 1300 feet below the level of the Mediterranean, and we are furnished with the most remarkable proofs of the mighty oscillations to which the surface has been subjected, even in recent epochs. In receding from the Mediterranean to the Dead Sea, the Lake of Tiberias marks the first depression, being 328 feet beneath the sea, and from this lake to the Dead Sea the declination is nearly 1000 feet! A few years back only and we were startled at the announcement, that the level of the Caspian Sea was 300 feet below the Mediterranean; and more accurate measurements have, indeed, reduced the depression to 82 feet; but that any cavity on a portion of the present surface should be 1300 feet beneath the level of the adjacent seas, proves an amount of vibration within a limited area, which is truly astonishing*.

Palæontology.

Ichthyology.—Geologists who have commenced their career since the glacial theory has been in vogue and have read the numerous memoirs and heard the exciting discussions to which it has given rise, are chiefly acquainted with Professor Agassiz as one of its most ingenious expounders. I have now the pleasure to acquaint you that M. Agassiz is once more completely absorbed in his great work on fossil fishes—that work which you so justly honoured, in the year 1835 to 1836, with your Wollaston Donation and Medal. Of his progress in this arduous undertaking, he has recently given substantial proofs, in the description of many ichthyolites of the Old Red Sandstone of Scotland; and, in addition to this, he will shortly publish a series of fossil fishes, exclusively illustrative of the tertiary basins of London and Paris, from which an enormous number of species has been collected.

In reference to the geological researches of my friends and myself in Russia, I must here state, that as it is our one great object to place in correct parallel the Palæozoic types of Russia with those of the other parts of Europe, we could not hesitate in referring all our Russian ichthyolites to Professor Agassiz; for whilst it must be acknowledged that Russia contains naturalists of great merit, and that among them M. Pander and Professor Asmus had commenced inquiries into the nature of these fossils, it was obvious that, skilful as they undoubtedly are, they could not, for want of comparisons, afford us the knowledge of which we stood in need. Pro-

* See the last discourse of Mr. W. Hamilton, the President of the Royal Geographical Society, who points out this admeasurement as being at length fixed by the admirable trigonometrical survey of Lieut. Symonds, whose calculations of 1311 feet approach very nearly to the still higher estimate of M. Berthou, who, from barometrical observations, placed it at 1332 feet.

fessor Agassiz, who has at his disposal fossil fishes from all those parts of Europe, the geological structure of which has been well explained, was alone capable of answering the following query; To what extent do the ichthyolites of Russia, which lie in beds superior to the Silurian rocks, and which are surmounted by the Carboniferous limestone, resemble those with which we are so well acquainted in Scotland and England? His reply has indeed been most satisfactory.

So complete, says he, is the identity of about ten species of the Scottish and Russian strata, that the specimens from the two countries may be confounded. Among them the *Holoptychius nobilissimus*, three species of the *Dendrodus* (Owen), *Diplopterus macrocephalus*, are forms which might strike any good observer, as they have been previously published by M. Agassiz; but from the more perfect specimens of other species which we brought from Russia, he has been enabled to recognize the presence in Scotland of the species of a common Russian genus, the *Glyptosteus*, and also of that gigantic genus the *Chelonichthys*, to whose remains I have before directed your attention as having been recognized to be of ichthyic character by Professor Asmus. To this enormous fossil fish, some of whose thoracic bones are as large as the breast-plate of a well-grown warrior, and a single bone of which measured nearly three feet in length, Professor Agassiz has given the name of *Chelonichthys Asmusii*; and he now informs me that he possesses fragments of the same creature from the north of Scotland. The knowledge of this fact will doubtless lead to redoubled activity on the part of Mr. Hugh Miller and those Scottish naturalists who inhabit the shores of the Cromarty and Murray friths, to produce a rival of the Russian giant; a hope which I cannot express without deeply lamenting the death of a most successful explorer of these remains, whose loss geologists have to deplore, in common with every one who could appreciate her range of thought, her accomplishments, and her goodness*.

The results, however, of the examination of the Russian ichthyolites go still further; for, on submitting to the microscope of Professor Owen some teeth similar in outline to those of his genus *Dendrodus*, he discovered in them precisely the same dendridic disposition of the vascular canals as that which led him to establish the genus from Scottish fossils. Nor does the value of this application of the microscope stop here, for Professor Agassiz has informed me, that availing himself of the weapons which Professor Owen had so skilfully wielded, he has commenced a series of researches, not only into the teeth but also into the structure of all the hard enamelled bones of the Russian fossil fishes, by which he will be able to show the same distinction in the other bones of the genera of this class, which Professor Owen has successfully established in relation to the hard parts of the higher order of animals. In such hands, therefore, the microscope has become an instrument of great utility in identifying fragments apparently obscure; and, as

* Lady Gordon Cumming.

it has been applied to the shells of Mollusca, and even to the lowest links in animal life, as well as to fossil plants, the geologist has thus acquired a new and powerful auxiliary. I am here, however, treading on ground now fortunately occupied by the Microscopical Society, the active promoters of which are well entitled to our gratitude.

Ornithichnites.—To American geologists we are indebted for our acquaintance with this new class of phænomena. The existence of the fossil bones of birds of ordinary size had, it is true, been ascertained by Dr. Mantell in the Wealden strata, but great was our astonishment, and I may add our incredulity, when Professor Hitchcock first announced, that in rocks of considerable antiquity (the exact age of which is still uncertain), there existed innumerable impressions in successive layers, which must have been formed by birds, some of them of gigantic size, and to which he boldly assigned the name of "Ornithichnites." Various opinions were entertained, and much scepticism prevailed concerning these impressions; but it is due to Dr. Buckland to state, that he never doubted that the views of Professor Hitchcock were founded on true natural analogies, and he accordingly published this opinion, with illustrative plates, in his Bridgewater Treatise. The recent visit of Mr. Lyell to North America, and a memoir he has read, as well as a communication from Dr. James Deane of Massachusets, have necessarily brought this highly interesting subject again before us; whilst a very remarkable discovery in natural history has at all events almost entirely dispelled scepticism regarding the true *bird-like character* of even the largest of the footsteps, however difficult it may be to imagine the presence of such highly organized creatures at a very early period. The observations of Mr. Lyell completely support the views of Professor Hitchcock as to the littoral nature of the footstep deposit in Connecticut, and that the prints in question were left by birds on the mud and sand of former estuaries, the bottoms of which were gradually submerged, and by the increase of fresh matter were permanently preserved.

Mr. Lyell illustrates the ancient phænomena by reference to impressions which he saw forming at low water on the mud of the sea shore of Georgia by racoons and opossums, and covered by blue sand before the flow of the tide, as well as by the recent footsteps of birds in the red mud of the Bay of Fundy, which if submerged would realize a complete analogy to the fossil footsteps through many successive laminæ of deposit*. He also believes with Professor Hitchcock, that the strata in question had been elevated and tilted since their original deposition, and he connected these movements with the evolution of trappean rocks, which in some places invade the Ornithichnite beds. In regard to the age of these beds no decisive opinion has yet been expressed, though they are referred

* A striking explanation of appearances, respecting which we were at first equally incredulous when pointed out on the surface of the Red Sandstone of England as fossil rain-drops, is given by Mr. Lyell in the actual formation of similar markings produced by rain on the mud of the shores of Long Island.

to one of the older secondary rocks. However this point may be determined, and I will presently allude to it, the great question remained to be settled; how induce us to believe that the largest of these footmarks were made by birds? Is it not unsafe to call in the presence of creatures of such high organization when researches all over the world have taught us that in rocks of far less antiquity no traces of the bone of a bird or mammal have been found? May not the impressions after all be those of some singular Sauroid animal with trifid feet, of which we have no links in existing nature? Looking to such a possible explanation, and reflecting on the striking interference with the opinion heretofore very generally received, that a succession from lower to higher orders of creatures was invariably evidenced in ascending from lower to higher deposits, I candidly confess, that nearly up to the present moment, despite of the clear and faithful descriptions of the facts, I have clung to the idea, that the markings would not eventually be referred to the action of birds. My scruples as a geologist have, however, I confess, been much shaken, if not entirely removed, by a discovery in natural history, which I do not hesitate in characterizing as one of the most remarkable of modern times.

From the examination, in 1839, of a single fragment of a bone brought from New Zealand, Professor Owen, though at first startled by its enormous size, at length pronounced it to belong to a gigantic form of the lowest organized bird, analogous to the diminutive Apteryx of the same island, in which the lungs approach more closely than in any other bird to the structure of those in reptiles. To this monstrous winged animal he assigned the name of *Dinornis*, and many of its bones, in a very perfect condition, having been subsequently found in New Zealand and deposited in the museum of the College of Surgeons, his opinion has been completely confirmed*. When it is known that the tibia of this bird is so huge that the femur of the Irish giant is of pigmy dimensions when compared with it, some conception may be formed of its entire size, which must have far exceeded that of the ostrich†.

Now to apply this discovery to our Ornithichnites, one of the great difficulties which many of us had to overcome was the gigantic size of the largest American footsteps, which measured fifteen inches in length; and it is a most curious fact, that upon placing the fossil cast alongside of the metatarsal bone and tibia of the largest individual of Dinornis, Professor Owen is of opinion, that if the feet of this great tridactyle bird be found, they will, from the usual proportions maintained in such animals, be fully as large as those of the American Ornithichnite. From this moment, then, I am prepared to admit the value of the reasoning of Dr. Hitchcock,

* The inhabitants of New Zealand believe that the Dinornis was in existence with their progenitors. On this point, however, doubts may still be entertained, as we know that in many uncivilized countries, where the bones of extinct quadrupeds occur, the natives connect them with their ancestors.

† See a most graphic sketch of this monstrous bird and its analogies from the pen of my friend Mr. Broderip, Penny Cyclopædia (*Unau*).

and of the original discoverer, Dr. James Deane, who it appears, by the clear and modest paper lately brought before us by Dr. Mantell, was the first person who called the Professor's attention to the phænomenon, expressing then his own belief, from what he saw in existing nature, that the footmarks were made by birds. Let us now hope, therefore, that the last vestiges of doubt may be removed by the discovery of the bones of some fossil Dinornis; and in the meantime let us honour the great moral courage exhibited by Professor Hitchcock, in throwing down his opinions before an incredulous public*.

Still, however, comes the question, what is the age of the rock on which the Ornithichnites have been impressed. Consulting what Mr. Lyell has recently written, we find that he does not decide this point further than by saying, that they were formed between the carboniferous and cretaceous epochs, the only remains hitherto found in the deposit being ichthyolites of the genera Palæoniscus and Catopterus, with some fossil wood. The presence of a Palæoniscus would lead me to suspect that the deposit might be of the age of the Zechstein or Magnesian Limestone; for in Russia, wherever the calcareous matter which represents that rock thins out, vast tracts are occupied by marls, sandstones and conglomerates of red, green and white colours, which form the Permian system, and in these beds Palæonisci occur. If the fossil fishes from both localities be placed in the hands of Professor Agassiz, and a comparison be made of the fossil wood from Russia and North America, the query may be satisfactorily answered. In the meantime I cannot read the descriptions of this American deposit, and carry the Russian types in my recollection, without surmising, that in the sequel the Ornithichnite and Palæoniscus beds of Connecticut and the gypsiferous rocks of Nova Scotia, distant as they are from each other, will be found to belong to one natural group—the Permian; and if this view be borne out, it follows that a bird analogous to the Dinornis lived at a period when Saurian animals first began to appear upon the surface, and when the last links of primæval or palæozoic life were not obliterated †. In this case the value of the philosophic caution given by Mr. Lyell will be very apparent, viz. that we ought not to infer the non-existence of land animals from the absence of their remains in contemporaneous marine strata ‡.

Saurians, *Cetaceans*, *&c.*—I am not aware that researches of the past year have added much to our acquaintance with new forms of vertebrata in the secondary deposits, though it must not be unrecorded, that our zealous contributor M. Hermann Von Meyer, has added to the list of Saurians of the Muschelkalk a new genus, which he describes under the name of Simosaurus.

In Russia a very curious discovery has been made by Professor

* See Geol. Proceedings, January 1843, Dr. James Deane 'On Ornithoidichnites.' [An abstract of this paper will appear in a future Number of Phil. Mag.]

† This view has been strengthened by the researches of Mr. Logan, see note, p. 546.

‡ Geol. Proceedings, vol. iii. p. 796.

Brandt, of which I have been just informed by my friend Count Keyserling. Pallas had spoken of a locality among the cliffs of Taman, in the southern steppes, where remains of whales were found; Rathke had mentioned the head of an animal of which the vertebræ were known, and which he described as approaching to a whale; and more recently Professor Eichwald considered this fossil as belonging to the Dolphins and named it *Xiphias priscus*. Obtaining possession of the specimen for the museum of St. Petersburgh, Professor Brandt worked the head of the colossal creature out of the rock in which it was imbedded, and pronouncing it to belong to a new family of whales, described it under the name of *Cetotherium Rathkii*. This fossil whale forms a new link in the animal kingdom, and is more nearly allied to the herbivorous cetaceans than to the Dolphins. Its position in the geological series is also most remarkable; for the rock in which it occurs contains shells similar to those of the tertiary deposits of the Miocene age, which extend from Volhynia and Podolia to the Crimea and Taman. It is also very remarkable, that along with this herbivorous cetacean the other organic remains (among which, however, banks of corals occur) have more the character of the inhabitants of a brackish sea than those of the subjacent rocks, whose fauna more resembles that of the Black Sea and the Mediterranean.

These relations are in accordance with modern conditions, and are, indeed, explained by an analogy in our own country, for an acquaintance with which I am indebted to our Curator, Mr. E. Forbes. The lake of Stennis, in the Orkney Islands, celebrated by Sir Walter Scott, has been converted, whether by elevation of the land or other cause, from a saltwater loch into a freshwater and marshy tract, and with this great but gradual change, certain marine genera have continued to live on amid their new associates of land and fresh water, whilst others have perished. That which is taught on a small scale in the Scottish lake has occurred over a vast area in the case of the Caspian Sea; which, in consequence of separation from the Black Sea has passed into a brackish state, and the same hardy and time-serving marine genera as in Scotland have continued to exist in their new abode! The Miocene deposit of Taman, therefore, with its herbivorous cetaceans, brackish and marine shells, is only an example, in an earlier period of the world, of the formation of a true Caspian, the creatures in which necessarily differed from those of the pure marine period which went before them.

MASTODONTOID AND MEGATHERIOID ANIMALS.

For a season our metropolis contained within it a magnificent skeleton of a Mastodontoid quadruped, which, in common with all geologists and palæontologists, I hoped to see permanently established in our national Museum. This gigantic animal was discovered by a persevering Prussian collector, M. Koch, who for some time resided in the United States, and who disinterred it, together with a great profusion of heads, teeth, and numerous bones of similar animals,

from amid the alluvia of a tributary of the St. Louis river, where the chief remains had probably been an object of superstitious tradition on the part of the Indian tribes. It does not appear whether the zealous Prussian had any scruples to overcome; but I presume they must have been considerable, if I were to judge from my own experience in other wild countries. In travelling along the eastern flanks of the Ural Mountains, it was my lot to visit many sites of gold alluvia in which bones of the mammoth and other extinct quadrupeds are found, and for these remains the poor Bashkirs, the original inhabitants of the tract, preserved so deep a veneration, that in freely permitting the search after the true wealth of their country which they were incapable of extracting, their sole appeal to the Russian miners was, "Take from us our gold, but for God's sake leave us our ancestors."

Overcoming, however, all difficulties, M. Koch succeeded in extracting, and afterwards in setting up, the most complete specimen of the species which has ever been seen. Applying to it the provisional name of "Missourium," he exhibited it for some time in the United States, and then brought it with many of the associated bones to London, in the hopes both of having the remains perfectly described and of obtaining for them a price worthy of the British nation.

The arrival of such a collection could not fail to excite the most lively interest and curiosity among our naturalists, and the bones having been attentively examined by many members of this Society, produced a diversity of opinion respecting the generic character of the chief remains. North America had long been a fertile mine of such reliquiæ, and the naturalists of the United States had not been backward in studying and describing them. It is not, therefore, a little remarkable that the same difference of opinion as to the generic and specific identity of the animals that prevailed across the Atlantic, is presented in the Memoirs which have recently been read before us; Dr. Harlan and Mr. Cooper having maintained opinions, with which, to a great extent, Professor Owen concurs, whilst Dr. Grant and M. Koch have supported the views of the late Dr. Godman.

Citing the American authorities on his side of the question, including Dr. Hayes, and enumerating no less than thirteen species of Mastodon and six species of Tetracaulodon, Dr. Grant has made a vigorous effort to vindicate the true generic characters of the Tetracaulodon as founded on the presence of a tusk or tusks in the lower jaw and certain variations in the form of the crowns of the molar teeth.

This view has been sustained by Mr. A. Nasmyth in an elaborate paper "On the Minute Structure of the tusks of extinct Mastodontoid animals." Microscopical examination of portions of the tusks believed to belong to five distinct species, viz. *Mastodon giganteus*, *Tetracaulodon Godmani*, *T. Kochii*, *T. Tapiroides* and the *Missourium*, has also led this author to the same inference as Dr. Grant; and he concludes with the remark, that, if it be established that specific differences positively do exist among all these animals,

the value of such microscopic researches is great; but if the five animals are grouped as one, then such mode of observation is of no value in palæontological science.

Professor Owen had previously expressed opinions at variance with those of Drs. Hayes, Godman and Grant and Mr. Nasmyth, and his views have been supported within these walls by my predecessor, Dr. Buckland. Pointing out certain mistakes in the setting up of the Missourium, as exhibited in the Egyptian Hall, he compares the fossil with all forms with which he was acquainted; and, showing that it must have belonged to the Ungulata, he judges that the enormous tusks of the upper jaw constitute it a member of the proboscidian group of pachyderms, and that the molar teeth prove it to be identical with *Tetracaulodon* or *Mastodon giganteus*. He argues that the genus Tetracaulodon was erroneously founded upon dental appearances in the lower jaw of a very young proboscidian, and that Mr. W. Cooper was correct in suggesting that the Tetracaulodon was nothing but the young of the gigantic Mastodon, the tusks of which were lost as the animal advanced in age. A comparison of the whole of M. Koch's collection produced the result in Mr. Owen's mind, that, with the exception of a few bones of the *Elephas primigenius* (Mammoth), all the other remains of proboscidean pachyderms in it belong to the *Mastodon giganteus*. The remains of other animals found by M. Koch are referred by the Hunterian Professor to *Lophiodon*, *Mylodon Harlani*, *Bos*, *Cervus*, &c.; and in respect to the *Mastodon giganteus* he expresses his conviction that it had two lower tusks originally in both sexes, and retained the right lower tusk only in the adult male. Although unable to form a correct judgement on the probable structure of those extinct quadrupeds, I may call your attention to a recent work of Mr. Kaup, whose striking discovery of the Deinotherium is familiar to you, and who now seems to advocate, from perfectly independent sources of evidence, the same views as Professor Owen concerning the osteology and generic characters of the Mastodon founded upon the comparison of a series of bones and teeth belonging to the *Mastodon longirostris*, more numerous and complete than even those of the *Mastodon giganteus*.

Mylodon.—One of the most brilliant, and, I venture to say, not the least durable of the researches in palæontology, remains to be mentioned in the description of the *Mylodon robustus*, a new species of gigantic edentate animal, accompanied by observations on the affinities and habits of all Megatherioid animals. After a sketch of the labours of Cuvier, who first described the huge Megatherium and pointed out its analogy to the family of Sloths and Armadillos, of the succeeding writings of Jefferson and Harlan upon the genus Megalonyx, of Dr. Lund on the Cœlodon and Sphenodon of Brazil, and of his own researches which established the Mylodon and Scelidotherium, Professor Owen proceeds to describe the megatherioid animal which he has named *Mylodon robustus*.

Of the purely anatomical descriptions, it is not my province to speak, and referring you to the work in which, through the en-

lightened munificence of the College of Surgeons, all the necessary illustrations have appeared, I pass to the generalizations, and learn that the Mylodon, in common with the Megatherium and Megalonyx, are genera of the family of *Gravigrada*, as distinguished from the *Tardigrada* in the order *Bruta*.

Professor Owen then proceeds to a comparison of the anatomy of the Mylodon with that of all analogous creatures, and after an able analysis, he satisfies himself, and also, I am persuaded, every one who has followed his close reasoning, that he has at length ascertained the true habits and food of this family of mammifers. From their dentition, it is inferred that the Megatherium and Mylodon must have been phyllophagous, or leaf-eating animals; whilst, from their short necks, the very opposite extreme to the camelopard, they never could have reached the tops of even the lowest trees. Cuvier, on the contrary, suggested that they were fossorial, or digging animals; and we all recollect the animated manner in which Dr. Buckland attracted us, whilst he described the Megatherium as a huge beast, which, resting upon three legs, employed one of its long fore-hands in grubbing up whole fields of esculent roots; a habit which procured for it the significant popular name of "Old Scratch."

Dr. Lund, a Danish naturalist, had considered the Megatherium to be a scansorial or climbing animal; in short, a gigantic Sloth. After a multitude of comparisons, Professor Owen rejects the explanation of all his predecessors. He shows that the monstrous dimensions of the pelvis and sacrum, and the colossal and heavy hinder legs, could never have been designed, either to support an animal which simply scratched the earth for food, or one which fed by climbing into lofty trees, like the diminutive Sloth; and he further cites the structure of every analogous creature, either of burrowing or climbing habits, to prove, that in all such the hinder legs are comparatively light. What then was the method by which these extraordinary monsters obtained their great supplies of food? The osteology of the fore-arm has, it appears, afforded answers which are valuable, chiefly for their negation of erroneous conjectures, such as that the animal was an ant-eater, rather than for the habits which it directly elicits. It is, therefore, to the organization of the hinder limbs that Professor Owen mainly appeals to ascertain the functions of the forefeet and the general habits of the Mylodon.

Arguing that the enormous pelvis must have been the centre whence muscular masses of unwonted force diverged to act upon the trunk, tail and hind-legs, the latter, it is supposed, formed with the tail a tripod on which the animal sat. Professor Owen supposes that the animal first cleared away the earth from the roots with its digging instruments, and that then seated on its hinder extremities, which with the tail are conjectured to have formed a tripod, and aided by the extraordinary long heel as with a lever, it grasped the trunk of the tree with its forelegs. Heaving to and fro the stateliest trees of primæval forests, and wrenching them

from their hold, he at length prostrated them by his side, and then regaled himself for several days on their choicest leaves and branches, which till then had been far beyond reach. After showing that from the natural inversion of the hind-feet the Mylodon approached to the scansorial animals, and thence inferring that it might have had climbing powers necessarily much limited by the other parts of its frame, Professor Owen states, that the inversion of the soles of the feet is least conspicuous in the Megatherium, whose bulk and strength would be adequate to the prostration of trees too large for the efforts of the Mylodon, Megalonyx and Scelidotherium. The Megatherium, in short, was the mighty tree-drawer, and had therefore no need of the adventitious aid of any climbing apparatus. Allow me to add, that, amongst other reasonings, those which lead to conclusions that one class of megatherioid animals was furnished with a hairy coating (like the Mylodon), whilst another, like the great Megatherium, was devoid of it, as evidenced by slight modifications of the bony structure of the hind-feet, appear to me to be not the least original and interesting.

Wholly incapable, as I am, to do justice to this masterly inquiry by the necessarily brief allusion which is imposed upon me by the nature of this discourse, I shall best execute my task in quoting the words with which Professor Owen sums up his reasoning.

"On the Newtonian rule, therefore, this theory has the best claim to acceptance; it is, moreover, strictly in accordance with, as it has been suggested by, the ascertained anatomy of the very remarkable extinct animals, whose business in a former world it professes to explain. And the results of the foregoing examination, comparisons and reasonings on the fossils proposed to be described, may be summed up as follows. All the characteristics which exist in the skeleton of the Mylodon and Megatherium, conduce and concur to the production of the forces requisite for uprooting and prostrating trees; of which characteristics, if *any one were wanting, the effect could not be produced*: this, therefore, and no other mode of obtaining food, is the condition of the sum of such characteristics, and of the concourse of so great forces in one and the same animal."

This, Gentlemen, is the true Cuvierian style, in which, as in numberless parts of his works, Professor Owen has continued to breathe out the very spirit of the founder of palæontological science.

It is by such labours that geology is steadily gaining a higher place among the sciences. Comparative anatomy has truly been our steadiest auxiliary, and well may we do honour to those who impart to us such truthful records; for, whilst the histories of the earlier beings of our own race are shrouded in obscurity, whilst the first chronicles of ancient Rome and Greece are now admitted to be exaggerated, and often even fabulous, we turn back the leaves of far more antique lore; and, not trusting to perishing inscriptions, mutilated by successive conquerors, and assuming a hundred meanings under the eyes of doubting antiquaries, we appeal only to the proofs in nature's book, and find that their reading is pregnant with evidences which must be true, because they are founded on unerring general laws.

In concluding this Address, I can assure you, Gentlemen, that, although not prepared without some labour, its composition has afforded me both gratification and instruction. Had I not felt a strong obligation to fulfil my duty, I should necessarily have been absorbed in the preparation of the work upon Russia to which I have alluded, and could not therefore have been imbued with an adequate sense of the vast progress which our science has recently made in all quarters of the globe.

The chief aim of this Society has been to gather sound data for classification; and, following out this principle, I have endeavoured to show, how the order of succession established in our own isles, is now extended eastwards to the confines of Asia, and westwards to the back-woods of America. From such researches, and by contributions from our widely spread colonies, we have at length reached nearly all the great terms of general comparison.

Besides ascertaining where the great masses of combustible matter lie, we can now affirm, that during the earliest period of life, conditions prevailed, indicating a prevalence over enormous spaces —if not almost universally—of the same climate, involving a very wide diffusion of similar inhabitants of the ocean. We have learned, that in the earliest of these stages of animal life, no vestige of the vertebrata has yet been found, whilst in the succeeding epochs of the Palæozoic age singular fishes appear, which, in proportion to their antiquity, are more removed from all modern analogies. In each of these early and long-continued periods, the shells preserving on the whole a community of character, differ from each other in each division—and in that later formation, where a very few only of the same types are visible, they are linked on to a new class of beings, the first created of those Saurians, whose existence is prolonged throughout the whole Secondary period; whilst we have this year seen reason to admit that even birds (some of them of gigantic size) may have been the cotemporaries of the first great lizards. With the close of the Palæozoic æra we have also observed a gradual change in the plants of the older lands, and that the rank and tropical vegetation of the Carboniferous epoch is succeeded by a peculiar flora. In the next, or Triassic period, we have another flora, whilst new forms of fishes and mollusks indicate an approach to that period when the seas were tenanted by Belemnites and Ammonites, marking so broadly these secondary deposits with which British geologists have long been familiar, and which, commencing with the Lias, terminate with the Chalk. And lastly, from the dawn of existing races, we ascend through successive deposits gradually becoming more analogous to those of the present day, until at length we reach the bottoms of oceans so recently desiccated, that their shelly remains are undistinguishable from those now associated with Man, the last created in this long chain of animal life in which scarcely a link is wanting !—all bespeaking a perfection and grandeur of design, in contemplating which we are lost in admiration of creative power.

Such results, grand as they are—nothing less in short than the

records of creation—are however but a portion of the labours of geologists. They have also struggled to explain the causes of those great revolutions. In some continents, it is true, the pages in the book of nature are, as it were, unruffled; for, by whatever agency effected, it is certain that beds of vast ancient oceans have been so equably elevated and depressed, and again so steadily elevated from beneath the sea, that the continuity of their rocky deposits over areas larger than our kingdoms of Western Europe is unbroken, and their original condition almost entirely preserved. In other regions, on the contrary, the sediments in the sea and the masses of the land have been pierced by numerous outbursts of igneous and gaseous matters, accompanied by violent oscillations and breaks, whereby the chronicles of succession have been sorely defaced, and often rendered more illegible than the most carbonized of the papyri found under the lava of Vesuvius. Nay, so intensely has this metamorphism operated, that obliterating all vestiges of former life, and concealing them from us, we have been sorely puzzled to ascertain by what powerful physical agency such mighty changes can have been accomplished,—changes by which the strata have been convoluted into forms grotesque as the serpent's coil, inverted in their order, or shivered into party-coloured and crystalline fragments. And yet in these broken and mineralized masses, as another branch of our science teaches, are found the precious ores and the metals most useful to mankind.

Such complicated relations and such changes in original structure call forth the application of the highest powers of physical science; not only involving the agency of that great central heat, to which geologists have willingly referred, but also invoking the aid of agents, some of them still mysterious, by which electricity and magnetism are bound together in the cycle of terrestrial phænomena. To few of us is it given to venture with firm steps into that region; and, though I hope to live to see some of these questions answered, I am well satisfied to have been among you when such solid advances have been made, in deciphering the mutations of the surface of the earth, and in the compilation of a true history of its earlier inhabitants.

Having now, Gentlemen, completed the term of my service, I bid you farewell, as friends in whose society, whilst acquiring knowledge, I have passed the happiest days of my life. Large as our numbers are, and branching out, as our inquiries do, into all the paths of philosophic research, the Geological Society has always held firmly together by a principle of good and high feeling among its active members. I have, indeed, deeply felt the honour of presiding over men who, in the course of a quarter of a century, have demonstrated, that there is no such thing as "*odium geologicum*," and whose members, rivals as they must be, have only sought to excel each other in their ardent search after truth.

By the choice of my successor you cannot fail to perpetuate this good feeling, for in him you recognize the philosopher, who, passing through other phases, returns to the object of his first love. In him

you applaud one of the founders of your Society, a munificent supporter of geological works requiring assistance, one of your earliest contributors, and one, I will add, of the best Secretaries you ever had—whether as respected the performance of his own duties or the singleness of mind and integrity of purpose with which, abjuring all personal considerations, he improved the Memoirs of various writers which found their way into your Transactions. His fitting reward, therefore, is this Chair, which I resign to him in the full persuasion that he will view it, as I have done, in the light of the highest honour to which a geologist can aspire; and that as one of our old and sincere friends, he will ever be imbued with the strongest motives for preserving the harmony and prosperity of the Geological Society.

ROYAL ASTRONOMICAL SOCIETY.

March 10, 1843.—The following communications were read:—

1. Occultations observed at Port Royal Dockyard, Jamaica. By Capt. Alexander Milne, H.M.S. Crocodile. Communicated by the Rev. Geo. Fisher.

2. Observations of the Beginning and End of the Solar Eclipse on the 8th of July, 1842. In the Fort of the left bank of the Shanghaie River, near to the Town of Woosung, on the Coast of China. By Capt. Sir Everard Home, Bart., F.R.S., H.M. Ship North Star. Communicated by the Rev. Geo. Fisher.

The observations contained in these two communications are stated in the Monthly Notices of the Society, No. 29 of vol. v.

3. Translation of a Letter from M. Hansen to R. W. Rothman, Esq., accompanying a copy of a printed paper on the Perturbations of the Heavenly Bodies moving in very Eccentric and very Inclined Orbits.

"Sir,—I have the honour to send you herewith the abstract of a memoir in which I have developed a method for calculating the perturbations of those celestial bodies which move in very eccentric and very inclined orbits. I beg you to forward this abstract to the Royal Astronomical Society. In this memoir I have treated only the case in which r is less than r', and I have simply adverted to the case in which r is greater than r' That I may be better understood, I add here that, in this case, it is the true anomaly of the perturbed body which must be employed. The integration is performed in this case in an analogous manner; but we cannot make the factors of integration depend on continued fractions: those factors depend in this case on a linear differential equation of the first order.

"Gotha, March 1, 1843." "P. A. Hansen."

4. On the Application of the Method of Least Squares to the Determination of the most probable Errors of Observation in a portion of the Ordnance Survey of England. By Thomas Galloway, Esq., A.M., F.R.S., one of the Secretaries of the Society.

The object of this communication is to give the results of an application to a part of the Ordnance Survey, of a general method of correcting the observed horizontal angles, whereby the positions of the stations are determined in such a manner as to give the nearest, or most probably accurate, representation of the whole of the ob-

servations. The method in question, which is due to Gauss and Bessel, has only recently been introduced into geodesy. In all the geodetical measurements which were executed prior to the latter part of the last century, the errors of observation were of such magnitude that it was unnecessary to take account of the curvature of the earth, and the triangles were accordingly computed as if they had been on a plane surface. On this hypothesis the sum of the three angles of each triangle is 180°; and as the strict fulfilment of this condition is necessary for the computation of the triangles, the universal practice was to apply arbitrary corrections to the observed angles, the observer usually assigning the largest correction to the angle which he *supposed* most likely to be erroneous. The large and excellent theodolite used by General Roy in his triangulation (begun in 1784) for connecting the observatory of Greenwich with the meridian of Paris, and the repeating circle of Borda, introduced about the same time on the Continent, gave the means of measuring terrestrial angles with far greater precision than had been obtained with the old quadrants, and the curvature of the earth became a necessary element in ascertaining the amount of the errors of observation. No alteration was, however, required on this account in the mode of correcting the angles, for as the *spherical excess* can be computed from approximate values of the sides to any required degree of exactness, the condition necessary for the computation of an individual triangle was still that the sum of the three horizontal angles should be equal to a known quantity; and in all the principal trigonometrical operations of which accounts have been published (excepting Colonel Everest's prolongation of the Indian arc of meridian, and some recent surveys in Germany), this condition (speaking generally) is the only one which has been attempted to be satisfied in computing the observations. But the observance of this single condition is not by any means sufficient to give the best representation of the *whole* of the observations: nor does it even suffice to give a determinate solution of the problem under consideration, for when the distance between two stations is computed through different series of triangles, each mode of computing leads to a different result. When the instrument has been set up at every station, and the angles between the other stations visible from that point have been all observed, other geometrical relations are established which the corrected angles ought likewise to satisfy, and angles are obtained which cannot be made use of otherwise than in satisfying such relations. Now, it is manifest that any mode of computing the triangles in which any observed angle is not taken account of, or any geometrical relation among the parts of the figure not satisfied, or which does not allow to every single observation its due influence, cannot be regarded as satisfactory. In order to obtain the nearest representation of the whole of the observations, or the result which is affected by the smallest probable error, it is necessary to solve the following problem, viz. to determine the corrections which must be applied to the observed angles in order that they may satisfy *all* the geometrical relations or equations of condition, and in order that the sum of the squares of the corrections may be an absolute minimum. A general solution

of this problem was given by Gauss in his *Supplementum Theoriæ Combinationis*, &c. (Gottingen, 1828), and the method has been applied by Bessel to the triangulation for the measure of the meridional degree in Prussia, and also to the computation of the extension of the French meridian through Spain, from Montjouy to Formentera.

The triangulation which has been selected in the present case for an example of the method, includes ten stations (commencing with the base on Hounslow Heath), at which thirty-five independent angles were observed. For determining the corrections of those angles, nineteen equations of condition are furnished by the observations, among which are instances of all the kinds which can occur in a trigonometrical survey. The final results differ extremely little from those given in the Survey, the greatest difference in the length of any side amounting only to about half a foot, and this in a distance of nearly eighteen miles. This close agreement must be attributed, however, to the smallness of the triangles, and the very great accuracy of the observations in this portion of the Ordnance Survey. If the distances between the stations had been two or three times greater, the observations would probably have been less exact, and the differences between the results of the two methods of computation more considerable; but however this may be, it is only by following the method here explained that the whole of the precision which is attained by the observations is preserved in the results; and for this reason the method should be adopted in all important surveys, particularly in those for determining the curvature of the earth. Besides giving a determinate result, and that result the one which is most probably nearest the truth, it has the great advantage of superseding all arbitrary corrections, and admitting only such as are rigorously deduced from the observations.

The methods of deducing the most probable values of the angles from the observations, of assigning the weights, of forming the equations of condition, and all the steps of the process to the final determination of the corrections by which the equations are satisfied, are given at length.

5. The President announced a communication that he had received from the Rev. Baden Powell, relative to an easy and convenient method of imitating the appearance of the *corona*, or glory, that surrounds the body of the moon, during the time of total darkness, in total eclipses of the sun; and also the appearance of the *beads* that occur not only in total eclipses, just prior to the time of total darkness, but likewise in annular solar eclipses. A sketch of the method was exhibited, which is merely this: a candle is placed in the focus of a lens fixed in a screen, with an aperture of about $\frac{3}{4}$ths of an inch in diameter, on the opposite side of which screen is placed an opake circular disc, of equal (or even greater) diameter than the aperture, which may be placed at different distances, so as to produce an eclipse of any magnitude, as the spectator shifts his position. When it is central and total, there is a brilliant ring, or glory, even when it is so much nearer to the eye as to subtend a much greater angle than the aperture. Also, when there are any cusps, minute irre-

gularities, on the edge of the disc, produce distinct *beads*. Professor Powell has tried a similar experiment with the circular opake disc and the rays of the *sun* reflected from a small piece of glass, which produced a most brilliant ring, the disc being nearly double the apparent diameter of the sun: and he proposes to pursue the inquiry still further.

MEETING OF THE ROYAL INSTITUTION.

Friday, June 9, 1843.

The lecture was by Professor Faraday, on the "Electricity of Steam." The Professor commenced his lecture by illustrating the extent of our knowledge of the electricity which accompanies the formation of steam previous to the observations about to be detailed, showing that when water is poured into a heated metal cup electricity was set free; and that if the vessel, into which the water was placed, was above a certain elevated temperature, no electricity was evolved in consequence of the water being prevented from contact with the containing vessel by a stratum of steam. The Professor then detailed the first observations made at Newcastle by one of the workmen in attendance on a boiler belonging to the Newcastle and Carlisle Railway, and whose report, that the boiler was full of fire, from the fact that when he placed his hand near it an electric spark was communicated, drew the attention of Mr. Armstrong to the subject, the result of whose investigations was then given. A boiler having been arranged for the purpose of illustrating this subject, the Professor exhibited the production of the spark during the emission of the steam, and showed most conclusively that the boiler and appendages were charged with negative electricity, while the issuing steam was in the opposite or positive state; that it was necessary that the boiler should be insulated; that the steam should issue through a small aperture; that the material of which this aperture was constructed modified materially the quantity of electricity, wood and the metals having been found by experiment to be the best fitted for the purpose; that the introduction of a small portion of saline matter, as sulphate of soda, into the exit chamber prevented entirely the elimination of electricity, and even when common water was introduced it had the same effect; that by long continuance of the issuing current of steam, electricity was gradually developed, from the condensed steam displacing and driving out the saline matter, pure water being a necessary element for its production; and that the whole phænomena arose from the rubbing of the condensed water against the tube from which the steam was issuing. The Professor also proved that the introduction of ammonia reversed the electrical states, what was before negative becoming positive, and that as the ammonia was expelled the original states were gradually restored; that turpentine and acids acted in the same way as saline substances, from their enveloping the particles of water in a film of their own substance. The lecturer considered, from these facts, that the view advocated by Mr. Armstrong, that the electricity arose from the passage of water into the aeriform state, was not tenable, and that the thunder-cloud and the lightning's flash could not be attributed to this origin.

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END OF THE TWENTY-SECOND VOLUME.

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